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THE

Psychological Review

EDITED BY

J. MARK BALDWIN
JOHNS HOPKINS UNIVERSITY

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(*Editor of the Psychological Monographs*)

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THE PSYCHOLOGICAL EXPERIENCES CONNECTED WITH THE DIFFERENT PARTS OF SPEECH.

BY

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THE PSYCHOLOGICAL EXPERIENCES CONNECTED WITH THE DIFFERENT PARTS OF SPEECH.

It has been observed by everyone that in literature of different eras, certain words vary in their form, become obsolete, and give place to new words, while others remain constant in their form. Moreover in the growth of a child's vocabulary, especially when he learns to read, it is noticeable that certain words afford him more difficulty than others. Quite aside from their length or difficulty of pronunciation, there appear to be classes of words which he adopts less readily than others into his vocabulary. To these general observations let me add one particular instance, that will furnish an illustration of the type of difficulty which prompted the investigation. The passage in 'As You Like It' describing the Seven Ages of Man is a familiar one. It will be remembered that the lines devoted to extreme old age run 'Sans eyes, sans teeth, sans taste, sans everything,' a passage which to me at least is always unpleasant. Although the necessity of a one-syllabled word expressing *without* is obvious, and we all know perfectly well what *sans* means, there is an invariable protest when the passage is read against any meddling with the preposition *without*. In spite of the familiarity of the passage, and the usefulness of a one-syllable word with this meaning, *sans* has never been adopted into our vocabulary. In fact we have no preposition-synonyms or alternatives, while new nouns are introduced every day. It was this fact that raised the query: What differentiates the prepositional state of mind from that which makes up the meaning of other words. In general, why are certain parts of speech easily adopted, while others defy all attempts to introduce them? It was from this simple starting point that the investigation took its beginning, and although departures into various other avenues were made necessary by the demands of the subject, the answer to this question in par-

ticular was the goal continually kept in mind. The experiments were made entirely on separate words, not on words in a sentence. The latter case would bring in a more complex state of affairs, involving much more than the consideration of isolated word units which is all that is attempted in this paper.

The first method of approach was to make out several lists of words, which comprised different parts of speech allied in meaning, and to get introspective information from the subject as to how her state of mind varied as she passed from the meaning of one word to the next. For instance, *entrance, enter, in, inner; weight, lift, heavy, under;* represent two lists with four parts of speech in each. The meanings involve the same general set of associations, and yet we have a distinctly different feeling for each word. My idea was, after having got all possible introspective notes on what the meaning consisted in to my subject, to see if the chronoscope would bring to light any difference in reaction time according to the part of speech. It might be that prepositions had a longer reaction time, *i. e.*, that the time necessary to know their meaning would show a variation from the time necessary to know the meaning of a noun, and that this would explain in part why the former were assimilated less easily. This at once brought us to a difficulty. My one subject (a senior in Mount Holyoke College), in the preliminary trials with a stop-watch, said she had no basis for giving a signal when she felt the meaning of a word. There were apparently various stages in knowing its meaning. Every word given had a familiar sound when first heard. Then images came and spread themselves out with more or less elaboration, and there was no point when she could say validly: "I know all that it means." If she reacted on the first familiar sound of the word, the difference between the parts of speech would be obliterated. If she waited for more data, what should be the arbitrary point at which she decided that enough associations had gathered about it to give it a real meaning? If she waited for enough deliberation to decide the question, the value of the reaction measurement was void. In general she noted what she called three stages in the growth of meaning.

(1) A feeling of familiarity with the word, that she *would* know presently what it meant (this stage of word meaning has been called Implicit Apprehension by Stout, in his discussion of this matter in his Analytic Psychology). (2) She then felt she would know how to use it, that is, the actual *meaning* came before (3) the images unrolled themselves in all their variety in the third stage. In other words, the images in the third stage, seemed sometimes to stay the same for two words of allied meaning, whereas she felt at once there was a difference in their meaning or their use. Although visual images were always present when she attempted to define meaning, they seemed arbitrary and not to express its essence. She had, so far as she could discriminate, exactly the same visual image for *drink* and for *water, i. e.,* a person drinking water in both cases, yet the difference in meaning was evident and remained the same with all kinds of voluntary changes of the images. For this reason, she felt that the meaning came with the second stage. The sound of the word was familiar, and *then* she knew what it meant, that is, she had a peculiar feeling of knowing just what to do about it, whereas the *images* appropriate to the occasion (whether pictures, the word written, or what not), although present when she attended to them, seemed more or less arbitrary. Of course in a sense any idea or any feeling is an image, and one might contend that the feeling of knowing how to use words was a memory-image of former use or something of the sort. But it seems to the writer that such a broad use of the word image, applying it to any possible mental state, simply vitiates its own particular significance. When image is used in this discussion it will refer to reproduced sensations whether of sight, hearing, touch, or any other; whereas feelings, attitude of like or dislike, tensions, etc., will be designated as such. The subject admitted that no definition could be given of the word without some visual or auditory images, some specialized associations. But she also insisted she felt what it meant before she could define it, even to herself, and that if she waited for images to elucidate the feeling, there was no determining where to stop. The more associations she had, the richer the significance which was draped on the skeleton, but the skeleton

was already there. Since this difficulty made any chronoscope reaction impracticable, we attempted an analysis of just what composed those stages in her apprehensions of word-meaning. If all words are prefaced by an immediate flush of familiarity, in what does this familiar feeling consist? The next portion of the discussion must elucidate this point.

After having given various words to the subject, all of which gave rise to an immediate feeling of familiarity, *i. e.*, she had known it before and would again presently, the question arose: Is this familiarity really part of the word meaning, or simply the reaction on the human voice speaking, regardless of what is said? Indeed when nonsense syllables were given, they seemed as familiar to the subject as the others. For a moment they were accepted as friends just as the others had been, but at once the content of the experience became 'I don't know what to do with it' instead of 'I do know.' Nevertheless there was an appreciable interval during which it was accepted because the general articulation and tone were familiar. Although the subject did not know how to react eventually on the syllable, that is, did not know what to do with it, she did know how to spell it, to reproduce it, and she was used to that tone of the human voice. Since, however, the feeling came equally well with words of real significance, foreign language or nonsense syllables, it could not be regarded as a part of meaning in its strictest sense. When, moreover, the words were spoken in a high unnatural tone of voice, this familiar feeling was retarded, and a feeling of peculiarity took precedence. After this there was the same familiar feeling as before. The familiar feeling here was apparently aroused by the *articulation* as distinct from the *tone* of voice. Now since a word to be spoken at all must be articulated to some extent, this feeling can never be wholly detached from the others. It must always be present to distinguish articulate speech from mere noise, but it cannot be regarded as part of the strict meaning, even though it is always present.

What then did the subject mean by saying she felt what it meant before any image was called up? Of course "what it meant," was the very question we were trying to answer, and

yet she felt she really grasped its meaning in the second stage of the whole word-experience. It was noticeable that the first image that usually came, was the written word or the sound of it. This image was so invariable that she called it a part of the second stage. At any rate, the image of the *word itself* came before any appropriate image of the *thing symbolized.* The image of the word itself, whether written or spoken, was invariable, while the images of the things symbolized were varied and arbitrary. Apparently part of the psychological experience of word-meaning is based on the word itself, apart from the thing it stands for.

The feeling of word meaning is apparently composed of knowing how to react on it to some extent. This reaction is at its lowest stage when the ability to react is solely to write or speak it, or even to give some approximate imitation of it in tone or articulation. This knowledge of reaction varies in complexity. It may be only slight knowledge of the general nature of reaction, *i. e.,* it is an imperative to do something, although just what is not recognized. Or it may be a rich complex of varied and discriminating associations. This necessary reaction means physiologically that the sound of the word has brought a train of associations with it and in going over into its centrifugal discharges, the number and extent of open channels varies largely. If nothing is known of the meaning of the word, but it is articulated by the human voice, the feeling of familiarity or vague consciousness of meaning (when in truth it has no ulterior significance) is merely the general feeling of the organism which accompanies the opening of the channels appropriate to reproducing the word by speech or writing. If the word was too complicated to be reproduced accurately, simply an approximate reproduction would be sufficient. There need be no actual felt tendency to reproduce, indeed no strain or sensation of any kind. But the very sensory stimulus of the sound must have some motor discharge before it can become a conscious state. The combined discharge of the associated auditory or written images which may be with it (more or less distinctly) gives a certain balance or *set,* to consciousness; that balance gives rise to its own peculiar feeling; and that feeling is the skeleton of

its so-called meaning. If on receiving the stimulus there was not even a reactive tendency to reproduce the word, the last vestige of its meaning as a word would be gone. We could then in no way distinguish it from mere noise. It might have practical significance, it is true, as the crackling of a tree would mean a limb was going to fall, or snapping in a fire-place would mean the fire had started. But not until there is a definite reaction to the sound on its own merits (not simply to what it points out) can the sounds or words be said to have a meaning. The reactive tendency to the word itself in its most barren form, simply the ability and tendency to reproduce it, and the feeling that accompanies this particular arrangement of open and closed motor channels, constitutes the first familiar feeling in the experience of its meaning. The meaning of a word is then a shifting term, not always signifying the same thing. Suppose a word in a foreign language is given us, of which we do not know the English equivalent. If we have never known it, the reaction is solely on its sound per se. Being a human utterance, in customary tone, it has a certain color of familiarity, which as we say it over, or have a mental image of it written or spoken, gives it a feeling of significance from habit. If one had never reproduced a sound or held a mental image of it in any way which prompted to reproduction, it would have no significance as a symbol. In fact when a child begins to regard the stream of talk going on around him as something which it is in his power to imitate, does not the feeling of significance in a symbol begin? Up to that time does a word have any meaning as a symbol any more than calling chickens or cows has a meaning for them?[1] The sound of 'co-boss' or 'chick chick' comes to have an inseparable connection with food; so do similar sounds with a child. When, however, the sound of a word calls up the tendency to reproduce it, it comes to have a vestige of significance.[2]

What then is the difference between the attempt to repro-

[1] *Story of My Life,* Helen Keller's first conception of meaning of language when she spelled *water,* p. 316.

[2] Stout, *Groundwork of Psychology,* p. 159.

duce inarticulate noises such as whistles, squeals, etc., and articulate speech? And what of the child who understands what is said to him long before he attempts to speak? In the case of spoken words a child learns as words those that have some invariable accompaniment. The word 'chair' is said when there is a chair in sight, 'gone' when something has disappeared from view. That is, the experience with the word is double; the word always accompanying the state of consciousness attendant upon a given experience. The fact of human articulation being so constantly attendant upon some 'otherness' of the experience comes to have an aura of its own. The very fact of a normal voice articulating something, no matter what, has so continually something else to accompany it (at first some sensational object, afterwards mental imagery of various kinds) that the feeling of otherness becomes inseparably attached to the articulating voice, and throughout life such a sound brings with it its shadow of significance, a reference to something else, even when there is nothing especial to point to. The articulate voice per se seems then to have a function of pointing, *i. e.*, of leaving us expectant and ready, even when there is no content pointed to. Everyone has the experience of listening to others conversing in an unknown tongue (Chinese laundrymen for instance), where there is not the remotest conception of the subject in hand, yet a feeling of significance accompanies the flow of uncomprehended talk. Those who do not understand French or Italian well enough to follow the connection in a play, will leave the theatre with the impression of having understood every word said. In these cases the expectancy, the pointing consciousness has been there, the feeling of otherness or of significance, although there was no subsequent content to fill it up. This feeling of familiarity has been given by Stout as the first stage of recognition of a known word, as if our familiarity were with that word, through our having known it before. I have attempted to show that this first impulse of familiarity is not connected with the particular word, but with human articulation, since the feeling is still there with an unknown or nonsense syllable. If the meaning of the foreign word has only been forgotten, and we recall

its equivalent, the expectant or significant 'set' of consciousness is supplied with a content, and it has a richer meaning. In fact some would contend that then only does it begin to have its meaning proper. But this feeling of significance in general, or 'otherness' than itself, is just as true a psychological part of the experience. Indeed where can we draw a line marking where true meaning begins if we rule out any of the experience? It must be all or nothing. Who shall say where actual psychological experience of the meaning of a word begins? Does it begin with the man who feels it may have significance of some kind, being articulated by a human voice? With him who knows it is a Chinese word? With him who knows it is a Chinese noun? With him who knows it is a Chinese word for an animal? With him who knows it is a Chinese word for a domestic animal? With the child who thinks it means some particular pet cat? With another who thinks it means all pet cats? Or with the man who knows it stands for the whole cat family? *Logically* of course the meaning would not be there until the two parties in the conversation agreed to have it stand for the same thing, no matter what that might be, or until the solitary thinker always insisted that it symbolize one definite idea throughout one course of thought. But psychologically, when we are concerned not with its ulterior purpose in a discussion, but are merely analysing the word per se to find what distinguishes it from a noise, we must not neglect any part of the experience, or strike it out because it is so constant a factor as to be habitually overlooked. We stated above that only when there was some impulse to reproduce, did we feel, strictly speaking, a sound as having symbolic meaning. If, for instance, we hear a bee buzzing in the room it means a bee is there; or if some one says 'A bee is buzzing in the room,' it means apparently the same thing. What then is the difference between the two? The word 'buzz' was originally an imitation of the sound. What differentiates it from the imitation of any noise which one might attempt to make, and what distinguishes the 'meaning' of the *buzzing,* when it points to the associated memory of the bee as making it, and when a person says it to *indicate* the bee? The essential difference is that the connection

between the bee and the buzzing is a non-voluntary, invariable one. The buzzing means a bee is in the room, but means so quite apart from us. We have no control over the buzz as a function of the bee. We may attempt to imitate the sound purely to satisfy the tendency to imitate any suggested sound. But such pure imitation, with nothing beyond it, is not expressive of meaning. If, however, someone says to us 'the bee is buzzing' we recognize a connection between that utterance and the attending fact, but the connection is not forced on us. It is voluntary. That is we *will* the connection in this case, and in the other the connection is quite out of our power. It would seem that the attempt to reproduce, however imperfectly or inadequately, is the sign of our recognition that it is a voluntary and not an inevitable connection. When one attempts to reproduce the sound of the cracking limb not in imitation solely, but in the word *crack,* which points to a certain experience, and which the speaker *wills* to have point to something, the symbolic meaning of the sound as a word begins. The various sounds then which are made to a child in the way of whistles, cluckings, murmurs, etc., since they have no invariable 'otherness' to which they point, do not arouse a sense of significance. Hence although a child may imitate them, they point to nothing further and do not afterward arouse the same invariable tendency to look beyond them for a content, that the articulate syllable does. They might have done so to be sure, and we might have had a language of varied whistles in which articulate syllables would have no symbolic associations. This, however, is not the case, and the feeling of otherness becomes the invariable associate of articulation, and the basis of our feeling of familiarity and significance with any syllable. This otherness may then be of two kinds, non-voluntary or voluntary, and we see that until there is a tendency to reproduce in some fashion, the connection is just as inevitable and non-voluntary between a fire and the word fire, as between the fire and its snapping. We may say then in summarizing that there is reproduction without symbolism in bare *imitation,* since the sound in this case does not point beyond itself, but stands on its own merits. Or there may be

a pointing beyond itself without symbolism, when there is inevitable, non-voluntary association as when the sound of rushing water calls up the idea of water. Until a child has caught the trick of trying to reproduce a word, it would seem connected with its object in the same way as the latter. When it tries to make the word while thinking of the object, it finds it, or *makes* it an object of its own will, and symbolism or meaning begins.

It seems important to emphasize this necessity of a sound being regarded as the expression of some *will* before it can call up the necessary state of consciousness in us which we call meaning or significance. I remember as a child the click of machinery on a train sounded to me like the articulated word "develop." Although I cannot get the illusion now at all, it seemed perfectly enunciated to me, and yet it had no significance to me any more than any other sound of machinery, because I did not recognize it as an expression of any will either my own or anyone's else. On the other hand, when my playmates invented cipher languages which I could not understand, they seemed significant nevertheless, because I recognized them as expressions of their wills. Moreover the last point to which this symbolic state of mind without any content could be stretched, was reached when we conversed together in syllables which meant nothing to any of us. These syllables were absolutely devoid of content or purpose, but since *we* were articulating them, the customary feeling of significance or pointing beyond, which habitually accompanies rational conversation, was transferred to them, and they seemed meaningful because expressive of ourselves. I do not mean we arrive at this through reasoning; it is an immediate experience. When a baby by chance enunciates some syllables that have a place in the language, they have no more meaning for me than if they *did not* happen to be in the language, because I do not feel that it is expressing itself. In a similar way the wandering phrases of a person in sleep, in so far as I feel he is not controlling them voluntarily, have no meaning. That is, in so far as I look upon the baby or the feeble-minded person as a 'thing' in the same sense as the machinery of the train, their syllables have no significance

whatever. In so far as they sound so much like the human voice, that the illusion of their expression of a will is strong, even though I know that it is really an illusion, just so far are they accompanied by the ghostly feeling of significance, the pointing beyond to another intelligence. While in the presence of a normal person of alien tongue, though his language be of the most grotesque syllables, and the meaning he tries to convey be quite without content for me, yet I meet his words and clothe them with all the warmth of my habitual feeling of significance into the spoken word, because I recognize it as a human expression of will. If I did not, there would be no difference between my attitude in face of him and in the face of a running brook that babbles because it must, not because it will.

The purpose of this paper was, as I stated at the beginning, to discuss the difference in our mental experience which made up the feeling for the different parts of speech. We found it necessary at first to analyze the nature of the reaction toward speech in general—we will now consider the specific differences as they come out in the subject's introspection.

I will go over the different parts of speech as we tried them. It will be seen that nouns can be described in terms of single images for instance, while a preposition could only be explained in a phrase of more or less complexity. If the subject, therefore, waited to make a clear statement to herself after each word, she could describe the noun-feeling in fewer words than the preposition, but she insisted she felt the meaning of the preposition so that she could use it, even before she could give an account of it. I took a noun, verb, adjective and preposition somewhat allied in associations, to find as one was spoken after the other, how the state of mind in face of them changed. In this way, since they had many features alike, those that were different would be the more significant.

Nouns.

Knife.—Images of knives. Slight tension as if to use knife. This is very slight, however, and only seems to supplement images.

Song.—Word written comes first. Then picture of some one singing, with emphasis on *hearing it, i. e.,* passive reception.
Weight.—Sees word written and has vague feeling of herself as a ponderous body.
Joke.—Couldn't detect actual tendency to laugh, but expansive feeling came with image of written word.
Book.—Image of books—pasive reception of stimulation which seems outside herself, not intimate.
Water.—Passive looking at stream. Meaning seems to arise from filling a place in consciousness already waiting for it.

Verbs.

Cut.—Impulse to cut with feeling of purpose. No image of the object of the verb, bare feeling of sharp cleavage of some kind with additional feeling of being purposeful agent or someone's being so.
Sing.—Saw word written. No impulse to sing, but in the image of singing person, the emphasis was put on act of doing it rather than hearing it. *Active* feeling.
Lift.—Feeling of weight, but less heavy than with weight, and more localized. There is was oppression in general, here, more definite in localization.
Laugh.—More definite adjustment than to joke. That was vague and unlocalized. This coincides with preliminary preparation to a voluntary laugh.
Read.—Feeling of herself somehow as *agent* in connection with book images.
Drink.—Some tension in mouth and throat. Can't tell whether due to meaning of or impulse to say it. Afterwards image of it written and of person drinking.
Drank.—Same image as before. Not active this time, no impulse to drink.

Adjectives.

Sharp.—Quicker response to word than to knife. The latter's meaning seemed more diffused and depended more on visual images. Sharp has no image except written word, but there is motor response in *cringe.* There is slight tendency

the same way after knife, but there it is only one of various associations and doesn't seem demanded, while for *sharp* the cringe is demanded as essential to its meaning—constitutes most of it. It is impossible to think *sharpness* when utterly relaxed. *Sharp* has not the purpose in view that the verb has. It is a reaction without purpose but with a decided feeling tone.

Sweet.—No image except word written, but slight smirk, especially when several phrases with the word in it are used. This is partly due to such a position of the mouth being necessary to say the word.

Heavy.—Image of written word. A more all-around feeling than weight or lift. The adjective seems more 'intimate,' to come closer home than allied words, although weight has an almost adjectival significance and reaction to it is quite similar.

Funny.—Meaning comes with written image. 'Broader' feeling than joke or laugh. Not so definitely localized. It seems to involve the whole person, but not in any definite way. More diffused and spread out.

Dull.—Stupid smothered feeling.

Wet.—Only image beside written word was dampness in the mouth.

Quick.—Sharp tension, especially when word is spoken quickly. 'Broad' feeling. Vague tendency toward something, but no image except written word. Tension without any purpose.

Prepositions.

Under.—Whole self involved in a tension toward something above it. Darkness overhead but not pressure. *Weight* had feeling of 'herself as bulk' for a content. *Under* refers to something outside herself. Slightly more localized in strain than of.

Through.—Vaguest impulse of whole self forward. No definition of purpose. Calls up other associations to complete its meaning, *i. e.,* the thing passed through. But by itself no content or purpose, only forward tension.

About.—Question whether any meaning at all is connected with these prepositions until some other associations are called up. May be only the feeling of familiarity with the sound. At most there is a diffused feeling of the whole self being about something, or something about it, *i. e.*, mere juxtaposition or concern with. No localization of effort and no purpose.

Feeling of tendency of spreading out. No image but written word.

Adverbs.

Sharply.—Not as intimate as *sharp*. Different significance than sharp. Figurative—not so localized. It points beyond to an action, but with less keenly felt force.

Sweetly.—Points beyond to an action, but word has figurative significance. It doesn't have an immediate sense quality as does the adjective, but involves some kind of judgment as an action. It seems farther off, not so closely associated with the self.

Heavily.—Felt in terms of someone's else action, while the adjective was in terms of herself as a ponderous body. Adverb more in terms of sound and motion as applied to an action, while the adjective was in terms of pressure.

Stupidly.—Farther away from herself than *stupid*. It seems to involve a judgment that something was done in a stupid way, but not an immediate feeling of dullness and inertness as was involved in the adjective wholly in terms of being a spectator or judge of someone's else action.

Quickly.—Feels in terms of action of someone else. It points to an action viewed and judged, but herself passive.

Pronouns.

He.—This leaves one in a state of suspense, of looking forward to some definite content. 'The man' involves the association of various qualities that go to make up our conception of and attitude towards men in general. "He" consists mainly in its pointing toward some man in particular. The very use of the word indicates some special individual in mind, and although it has no definite content, it

has for its function to point to a definite individual.[1] The proper name supplies definite content: The pronoun points to definite content and one feels that the next moment he will know 'Whom.' While the noun 'man' involves no attitude toward the individual with his personal peculiarities but to the type, 'He' has therefore a much richer reaction. There are expectant possibilities that will soon be realized in definiteness. It involves the certainty of an answer to the question 'Who?' This poise on the question and expectancy of answer is characteristic of the different pronouns.

The unclassified words of Yes and No can only be expressed in terms of action. We cannot think yes while shaking the head and going through the attitude of repulsion. The same is true of interjections such as lo, hark, etc.

What then are the general facts we gain from such a survey of reaction to different parts of speech?

When words are spoken separately without content, there is an invariable image of the word written or else an auditory image preparatory to saying it. This confirms our conclusion that the tendency to reproduce is a necessity if we are to appreciate the sound as having symbolic meaning. In a spoken sentence where the phrase as a whole is taken as the unit, further analysis would be necessary and modification of this statement. In that case, some purpose of the dialog as a whole, takes the place of the pure detached significance of word units, which is the only matter under discussion at present.

To begin with the nouns: It would seem that with the noun, the written image is only one of a number of different associated images that constitute the meaning for us. In the case of knife for instance there comes an image of some particular knife (the one probably most often used by the subject) plus the association of cutting, plus as many images of other knives or circumstances connected with them as may happen to come up. The meaning knife is the only one that can fill the gap towards which all these associations point, and moreover it is the constant associated factor of *what we should do with the object*

[1] Miller, *Relation of Function and Content in Mental Phenomena.*

in question that binds together the different associations. In other words it is a constant attitude binding together a variety of images, which make up the meaning of the noun.

A child who has never been allowed to use a knife, and does not have associations relating to its cutting capacity can nevertheless attach a meaning to the word by some other constant attitude—something to play with, something to steer clear of. In so far as, knowing nothing of its cutting function, closed pen knives mean objects to play with and carving knives mean something not to touch, the two attitudes make the same expression into two distinct words for him. They have no more in common simply because denoted by the same syllable than do *stock;* the article of neckwear; the goods a merchant has on hand; and the cattle upon a thousand hills. In any of these cases, however disparate the image may happen to be, whether pen knives or those used in quarrying stone, it is the constancy of the associated functional activity which binds the meaning. Any noun must involve some kind of motor attitude toward it. Whether it is the attitude in which the rest of the world would agree with us is immaterial (as when the moon means to a child an object to clutch at and to us a heavenly body) so long as it calls up some constant response in us. This constancy of response is all that ties the various images together and makes one word do for all. Otherwise a symbol for each would be necessary.

We have seen that some appropriate and constant attitude towards the object is necessary as one of the noun associations. Is it also necessary to have some sense image? Could one know the meaning of knife, bereft of every image except the motor tendency to cut? Such exclusive insistence on the function of the knife would deprive it of most of its noun significance and it would become more like an abstract phrase, 'something to cut with.' The very experience of a concrete noun involves associated images. It is the part of speech devoted to that universal habit of mental imagery especially visual. As soon as we ignore our images or for some reason have but few, our vocabulary is impoverished of concrete nouns. We may get on in conversation and in thought, but the psychological 'noun' state of mind becomes poorer. There would be a differ-

ence, however, between the meaning of the noun 'knife' and the verbal-noun 'cutting' even for one to whom the former brought no distinct images. The verbal noun necessitates the response toward something in the act of carrying out a purpose, although it need not and in fact seldom did (with my subject) involve the self as agent. The noun, although it involves as one of its necessary associations the knowledge of what it is for (at least what it is for so far as we are concerned), *i. e.,* what is our necessary attitude toward it, does not involve our attitude toward it as actually carrying out that function. In the case of the two words we have been using, we can feel the change from one to the other. 'Knife' involves to most of us the picture of whatever knife may happen to come, an auditory or visual image of the word, bound together by a feeling of harmony in the present attitude with the attitude before any actual sharp blade used for cutting. Thus the more or less rich associations which point to this word, plus the connecting link of what we would do if it were present, bring about a characteristic feeling, and that feeling is the meaning of the word. I am not using the knife, no one is; the association of its function is simply a passive knowledge of a functional possibility. With the verbal noun 'cutting' there is this difference. The function is no longer a possibility, but is being carried out. We may still feel no impulse to do it ourselves, *i. e.,* our attitude is passive, but it is as a spectator of something carrying out a purpose, instead of something that is only able to do so. There is in this case a cluster of associations more or less varied according to the individual, but the essential point here is the attitude toward an active purpose going on at the moment. The verbal noun may not indicate action (as in the case of 'resting' or 'lying'), but at any rate a purpose is being carried out, and it points to an agent, although with the noun the fact of agency was not involved. In general, the characteristic feeling of 'nounness' is of passive surveyance of means, without implication of end, or ourselves or anyone as agent, although the functional possibility of the object or idea in question is one of the associations (and a necessary one) that goes to make up the characteristic state of mind. The verbal noun does not have as

many associations, but the characteristic state of mind is made up of the attitude appropriate to something or someone carrying out a purpose. Some present purposive functioning with the implication of an agent, makes up the 'verbal nounness,' while other associations give the particular color to the word. The attitude here makes up more of the word meaning than with the noun. There it was only one of a complex of associations; here it is the principal one about which the others cluster. As in the other cases, images of some kind, as well as appropriate attitude are necessary, it is a question of emphasis on their relative importance which decides. It is an interesting variation of the experiment to take some meaningless syllable, and view it first as a noun and then by adding, *ing,* to view it as a verbal noun. For instance, the change from saying 'flum' to 'flumming' although both are without specific content, may still emphasize the characteristic verbal noun state of mind. That is, through constant association of this ending with a verbal noun, we still have a diffused feeling that someone or something is carrying out his purpose to 'flum' whatever that may be, while the noun associations are restricted to the word written or an auditory image or impulse to say it, and we feel we only need to know what to do with 'flum' to adopt it at once into the language. It is noticeable that 'verbal nounness' has more of a skeleton than a concrete noun. The latter needs more content in the way of images than the former, while 'verbal nounness' as the state of witnessing some purposive functioning has a sturdy structure before any special content is given it.

In what respect does a verb differ from the parts of speech already discussed? When different verbs were spoken to the subject, without the infinitive preposition to, they naturally came with the force of an imperative.[1] So that this feature of the word was especially emphasized. In almost every case with a verb, there was a definite impulse to carry out the suggested action, although occasionally there was an image of another person performing the act and of herself as spectator. With the verb there was much more tendency to consider one's self as agent carrying out a specific localized purpose, doing

[1] Dugald Stewart, *Phil. of Human Mind.*

it and getting done, while with the verbal noun, since the puipose was not fully carried out but continuously being done, it was naturally viewed from the outside as a state of purposive functioning, instead of an actual fulfilling of the purpose by one act. In the case of "drinking" one feels the attitude appropriate to viewing someone continuously carrying out the purpose to drink, although the end is not accomplished. There is no finality about it, and for this reason the attitude is more apt to be that of a spectator than an actor. With the verb, the attitude changes to that of a specific purpose accomplished. That is, there is not only purposive functioning of an agent toward an end, but the end is accomplished. Hence there is more of a tendency to feel it in terms of one's self. An act of will accomplished is more *intimate* than one operating but not attaining its end. Always operative, it becomes a state not an act, and although the purpose is implied in it, since it is never finished, we are more apt to feel it in terms of someone's else performance. With the verb, an end carried out is consonant with one whole voluntary life, and the tendency is to respond by some appropriate motor image or impulse, which indeed makes up the meaning of the word for us. A purposive action with the self as agent and with the end accomplished is characteristic of the verb. The verb may have a variety of images but the motor one of carrying out the act is the usual one, although the image of the agent is often someone else than the person himself. This is especially true in verbs representing actions not customarily carried out (steal, kill). Here the meaning may be solely in terms of one's attitude in view of someone else doing the act, although the implication of effective agency is present in either case. The question of possible difference between transitive and intransitive verbs will be touched upon later.

Adjectives are the next forms we shall analyze. Adjectives seem to be more intimate, more personal words than any yet given. Several phrases were used by the subject in explanation such as 'broader feeling than noun' 'seems to spread over the whole of me,' and it was noticeable that almost invariably some feeling-tone was connected with the adjective.

Although the adjectives were thought of as applying to something else, the meaning was felt in terms of their effect on the person. Thus, in all the phrases 'sharp wind,' 'sharp knife,' 'sharp rebuke,' 'sharp pain,' the abiding sense of sharp, although applied to different objects, meant a subjective cringe. The likeness of all these various nouns was solely in the fact that they produced the same kind of a shiver. The adjective state of mind is composed of a definite qualitative content. It involves no purposive action, no feeling of the self as agent and acting toward an end or of anything else doing so. It is concerned with subjective reaction regardless of what it acts on or any end to be accomplished. The adjective has the function of pointing beyond itself to the fact of qualifying something. Adjectives differ in this respect, some referring to elemental sense qualities or to immediate judgment, have a very rich content and little of the character of pointing beyond. On the other hand, adjectives which merely express the abstract qualities of external things such as oaken, wooden, iron, etc., are so devoid of individual content, that they do nothing but point to the noun whose qualities they express. Thus the pointing character is true in varied degree of all adjectives, while those which have it to the largest degree, have nothing to express as different from the noun. Adjectives of this class have the adjective form of qualification, while they really are more like nouns. That is, the form is that of an adjective but the content is that of a noun. With the other adjectives expressing sense-qualities, judgments, etc., we can only feel them in immediate terms of subjective reaction and although we apply the term to an external object, the word gets its significance from a subjective feeling which we transfer to it. Here the distinct function of an adjective as pointing beyond, is less noticeable since it has such a rich content of its own. The one type is adjective in form, the other more especially in content. It is noticeable, moreover, in this connection that the adjectives are popularly supposed to have opposites, and that one cannot feel the meaning of both at once. Thus hard and soft, wet and dry, good and bad, fast and slow, etc., commonly express opposites, while nouns, pronouns, verbs or verbal nouns cannot do so. There

can be no opposites in objects, neither can there be opposites in most actions, except to do or not to do them. Walking and running are no more opposite than walking and sitting still, or walking and riding. In these cases the only possible opposites are walking and not walking. For any act so complicated as walking, no other act could be found which in every respect used the opposite set of muscles. How does the case differ with adjectives? It will be noticed in the first place that it is *not* altogether different with adjectives in respect to their opposition. For instance we do not feel any opposition between ultimate sense qualities, blue and red, green or yellow, neither is sweet opposite from sour any more than bitter. We cannot indeed say that hot and cold as bare sense qualities are opposite, nor is the awareness of pain opposite to anything. In other words, in dealing with qualities or sensations (as Professor Münsterburg has pointed out[1]) there is no question of opposition, it is only when we will have more or less of them that the question of opposition arises. This is the only possible opposition between two ideas—I want it or I don't want it, I accept or I resist it. Accepting one thing I preclude the possibility of rejecting it at the same time. In so far as some judgment is passed upon them, two things are opposite, we will have them or we won't—in no other respect can they be opposite. In the case of running, to be sure, the act does preclude the practical possibility of sitting down, of playing the piano, or of going to sleep, but this exclusion of possibilities does not oppose them. It is only when we say 'I will' or 'I will not' that opposition begins. With adjectives, moreover, since we have seen there can be no opposition in qualitative difference of feeling (and it is this that makes up the adjective state of mind) why is it that there is this popular opposition of adjective states? It is because such an intimate character is taken on by the adjective state of mind especially by those adjectives connected with abstract nouns, expressing as they do the qualitative differences of our language, that there is likely to be some *affective coloring* of the word experience. In so far as there is such, they are opposed. Moreover, in so far as there is the opposition of

[1] Grundzüge der Psychologie, p. 51.

expansion and contraction with two words, they can be classed roughly as opposites, although this will not always stand the test of analysis. There was a distinction brought out by the subject, which referred to certain adjectives being 'broad' or 'diffused.' She could not tell just what she meant by this except that adjectives more than other words seemed to involve the whole self, and moreover certain adjectives did this more than others. Now it is plain that only states which seem to involve the whole person can be called opposites. We found that blue and green simply being eye stimulations, could not be called opposites. We may like the blue and detest the green, and in that case the like and dislike are opposites, but the two colors remain amicably side by side. With white and black, there is some difference since brightness and absence of brightness bring about an opposite effect of expansion and contraction. With taste, sweet and sour have been habitually opposed, because the one implies a receptive relaxed attitude and the other an attitude of contraction and repulsion. Only in so far as this affection of the body as a whole is taken into consideration can sweet and sour be called opposite. Such adjectives as loud and soft or hard and soft, or rough and smooth, affect us more intimately than visual objects. They must come more closely home and hence arouse throughout some characteristic resistance or acceptance, and only in so far as they do this is there any meaning in their being termed opposites. In other words, any adjective which means the combined qualitative result of various sensations or muscular reactions, so that in some way the whole person is involved, either in accepting or rejecting, or in action with or without hindrance, such an adjective can have an opposite. Any sense quality definitely localized cannot have an opposite, although we may have likes and dislikes in regard to it. Any general state involving the whole person may also have an opposite, in terms of an opposed system of contraction or expansion, without opposition of feeling tone, *i. e.,* I may enjoy the resistance of the rough object, or the cramp of the low, although I feel it in terms of contraction in both cases. In proportion as a sensation is simple, it has so many different from it, that none could be called oppo-

site. For instance any color would have all the other colors to reckon with, as well as all the other sensation-qualities for however violent the like or dislike might be, that is not enough in itself to bring about this classification as opposites. As the different sensations combine and bring about a combinational reaction which is more and more complex, and involves more and more of the person, opposition becomes possible.

It is noticeable that in proportion as the adjective quality is simple and sharply localized, we tend, in hearing the word, to apply it to something external. This is true especially with visual qualities, and with others in so far as we can localize them clearly. When, however, there is no sharp localization, the quality is felt more especially in terms of one self. Round or square, for instance, can only be imagined in terms of sight or touch. In either of these cases we do not or cannot imagine them as affecting our whole persons, hence there is no opposite and we feel the adjectives in terms of an outside object. Hot and cold on the other hand are not localized definitely. We do not feel in terms of our separate temperature spots, we respond all over our skin, and therefore we feel hot and cold in terms of opposed expansion and contraction, and we feel it in terms of ourself.

We find then that the adjective function in general is that of expressing the intimate effect of stimuli on organism and at the same time of pointing to an object as possessing these qualities, and it is more likely than the other parts of speech to have an affective coloring. It must, if it has any meaning apart from its noun, have a definite content, which may be in terms of elemental sensations or of the reaction subsequent to these sensations, or a combination of both. When the whole person is felt as involved in the reaction and the sensational content is diffused over enough area, we classify the qualities as opposites, because of the opposite reaction they call up. Such adjective qualities are apt to be felt in terms of the self as experiencing them, instead of in terms of external objects. When, however, the stimuli are localized, and do not seem to affect our whole sensitive surface, the adjective qualities tend more to have their meaning as applied to something else, and we do not oppose them.

What shall we say then of prepositions? These seem to have the characteristic feature of the adjective in that many of them are found in opposed pairs, out and in, to and from, etc., but they certainly do not have, as do adjectives, an affective coloring. They seem singularly barren of any content at first, and an attempt to analyze the prepositional states of mind seems discouraging. Thus the dictionary definitions of the preposition *with* as 'Expressing relation of intimate connection,' and *of*, as 'Associated or connected with in some relation' are plainly inadequate to give one who did not know the word, an idea of its meaning. The words we have already considered (with the exception of the pronouns) have other words more or less synonymous, in which their meaning may be expressed by way of definition. But there are no synonyms for prepositions, and no other words which could be substituted in the case of any of them with any approach to sense. I might say for instance 'He shed tears' 'he wept' or 'he cried' and convey essentially the same meaning in every case. But what could I substitute for the preposition 'with' in 'Come with me'? Anything but *with*, would mean something totally different, and yet how can I define *with?* The prepositions are notoriously more difficult to teach children in reading than the other words and they are usually the last they learn to speak. There seems to be no other way of expressing the meaning of prepositions than by *tensions* of various kinds. One cannot define them in terms of anything, because there is nothing else to them but a variety of muscular tensions, which when no other words are given with them is quite without ideational content. This tension is not localized as with verbs, in any special part of the body, neither does it have as with verbs any purpose. As the subject herself expressed it (in the case of *in*) 'there seems to be a bare huddle without any purpose in view.' All the different prepositions can be expressed by some variety of 'huddle,' and indeed that is the only way they can be expressed and have any significance. It is because they have no meaning in anything beside this tension, that a dictionary has such difficulty in putting the meaning into words. A definition necessarily expresses one thing in terms of another. The 'other' in the case of verbs,

would be a description of a type of action similar in kind to the first, and bringing the same results. A noun definition would describe a similar object to the one defined, and one that performed the same function. An adjective definition would give other adjectives that brought us in general into the same state of approval or disapproval, of reception or resistance, or muscular preparation as the one in question. In the case of ultimate sense qualities it could only give the physical stimulus which would bring about the sensation. With a pronoun the meaning could easily be given as a word which stood for any noun that we intended it to stand for. But with a preposition there is a difference. We cannot define it in terms of things having similar purposes or ends in view, for a preposition implies no purpose until linked with an object. It cannot be defined in terms of similar objects, for a preposition stands for no external object. It cannot be defined in terms of words which bring us into the same affective state or attitude of judgment, for it has no coloring of this kind. Its only content in fact is a certain tension or motor impulse, which has no purpose, significance, affective tone, nor feeling of will or agency. It is as the subject said ' a purposeless huddle ' with a dumb pointing beyond of attachment to something else. Some prepositions are expressed by definite expansion or contraction of the whole person. For instance *in* and *out* have no content except as contractive and expansive tendency respectively, with a feeling in both cases of something else as concerned in the experience. That is I am vaguely conscious of crouching or getting free in regard to something, although I have no idea of what, and it is immaterial for the meaning of the word that I should. *With* means intimate reception of something; *of* is a vague diffused feeling of possessing or being possessed by, *i. e.,* motion toward an invisible something. In any of these cases, the only real content of the experience arrives when some definite image is added. But there is some variation of tensions for each preposition in respect to the unknown object to which it points in the background, and although it is felt distinctly in terms of one's self, it is not as an agent with any aim in view, but simply expresses the various ways in which the person and the world can

relate themselves. They express the simple attitude qualities, just as blue, green and red represent the simple color qualities, and warm and cold the simple qualities of temperature. We afterwards combine these simple qualities, color for instance, in various ways and then can define those combinations in terms of the simpler. But to define the elemental color qualities in any other terms but their own is impossible. So with attitude qualities. We are in relation with the world in every variety and combination of withness, withoutness, to and fromness, aboutness, etc., according to the end we have in view, or that others have for us. We can analyze and decompose these relations into the elemental qualities of attitude or relation, and beyond that we cannot go any more than we can define 'blue' in terms of anything other than itself, except the ether waves which are its physical equivalent. The reason that a child does not feel prepositions or adopt them is therefore plain. He does not separate himself distinctly from the world, nor external objects from each other. If he feels a preposition it is not as something separate from the noun. That is, he says the noun and supplies the *with* or *without,* etc., by his action, identifying that attitude for the time with the noun. To feel the preposition alone, involves an abstraction of one thing from another, for the preposition expresses the elemental character of the relation existing between them. Since a child does not abstract himself, or the different parts of the situation from each other, his prepositions are only felt as parts of the noun. That is, *on* would only be felt in particular cases such as apple-on-table, or ball-on-chair. The elemental character of the *on,* as distinct from the two things related is too abstract for him. He colors the noun with the appropriate prepositional relation quality, and only learns the relation is apart from the object at a later stage in his development. There is a similar difficulty for him in abstracting any elemental sensitive quality but it is more noticeable in the case of the elemental attitudes of the prepositions. To have abstract relation there must be two things, while in case of the abstract sensitive elements there need be only one. For instance, a child in learning the quality of 'blue,' does not have to abstract himself from the

object. His notion of himself as different from the object in question may be entirely wanting. The only idea is blueness, and although there is some difficulty at first in abstracting that quality from the particular object, the abstraction can be reached giving a number of different objects with only the one quality in common. With the preposition ' on ' there are two things to be kept apart in his mind; either himself and some other thing, or two objects. While the number of objects to be considered increases in arithmetic ratio, the difficulty of determining what is to be noticed in the relation between them increases in geometrical ratio. While any object may arouse only a limited number of sensitive qualities, its relations become very complex when it is put in connection with something else. The book is *on* the table, but so is the table *under* the book, both are *away* from the child; and in trying to explain to him the meaning of the particular relation *on,* he confuses it with the act of putting it there, with the question why it was put there, and many others. One can never point to one thing as illustrating a preposition in itself. It needs two to illustrate. Moreover a further abstraction is necessary. The two things related, while both have to be kept in mind, are not in the same relation to each, *i. e.,* while the book is on the table, the table is not on the book. The child in the case of two blue objects can abstract the blue quality, because it is the only feature the two have in common, and he needs then only to fasten its attention on the one quality of blueness. With the preposition *on* he must not only keep the two objects in mind as separate, but he must also recognize the relation between them as exactly *opposite* according to which object he considers. If he thinks of himself on the table, he is on as regards the table, but the table is under as regards him. To a mind as yet hardly able to distinguish himself 'as other than the thing he touches' it is no wonder that such a complicated state of abstraction is impossible. As I said before, what prepositional meaning he gets is merely as a modification of the other words supplied by appropriate action.

Adverbs.—It remains for us to consider the special differentia of adverbs. It will have been observed in the introspec-

tion given on the adverbs (which were allied in meaning to the list of adjectives) that a uniform difference in character was present. There was a distinct feeling, as each adverb was spoken after its allied adjective, of the meaning moving away from the subject. It seemed in every case less intimate, less highly colored; it takes on more the character of a judgment, a comment, on the action of something else, than an impression of an immediate feeling in terms of one's self. It will be seen at once that in certain cases this must be true. All the adjectives which apply to elemental sensations of sight, sound, taste, smell, temperature or pressure, express immediate sensitive qualities. These cannot be defined in terms of anything else, or be understood unless the observer has experienced them himself. These, moreover, refer to bare sensitive qualities with no action, no purpose involved. There can be no meaning in such adjectives made into adverbs, but a figurative meaning. That is 'to sing sweetly' is to sing in such a way as to put us into approximately the same state of mind as a sweet substance does. Its whole meaning is based on its producing a reaction similar to that produced by the adjective. But the first essence of the sensation 'sweet' is not there, it is second-hand. The action has not the quality sweet in its real sense, but only produces eventually in us the same state of mind. It therefore has lost the vigorous immediate sense quality of the adjective. Moreover, every one thinks of his own action in terms of motive or purpose, not in terms of its appearance. Therefore an adverb of this kind being used figuratively to express an apparent quality of the action, we tend to feel in terms of some one else rather than ourselves. The subject felt herself as the passive spectator of an action, which she called sweet. She could feel *sweet* in terms of herself, she must in fact, but since *sweetly,* referring to action, necessitated a figurative use of the word, it involved a judgment on action, not a motive, and she felt in terms of viewing some one else. This observation held true for sharply and heavily. Sharp involved a cringe, as of herself receiving a sharp stimulus; sharply was felt in terms of someone else saying or doing something, which affected her as a sharp instrument would. Heavy was felt

in terms of herself as a ponderous body; heavily pointed to some action that sounded or looked heavy, with only a faint coloring of pressure. This change of the sense involved, was probably due to the fact that the active purpose of someone else is more likely to reach us in terms of sound and sight than in pressure. With pressure words one would have to be involved very closely one's self to get their significance, and since the adverbial form is of itself less intimate in character, the tendency is to feel such adverbs in terms of sense qualities, through which one could receive an impression of the action of *another* (*i. e.,* sight or sound). It would seem then that adjectives expressing sensation elements involved no judgment in their meaning. We accept the stimulus, and give a name to the quality of the sensation, and this sensation can be defined no further except to point to the stimulating object. That is the last resort. If someone does not know what we mean by sweet, all we can do is to give something sweet to taste, and say 'taste for yourself.' Whatever sensation he got from the same stimulus we should have to call by the same name eventually, although we are forever debarred from finding out whether we really have the same sensation qualities. There is then an objective standard for our sensitive qualities as expressed in adjectives. But when it comes to the allied adverbs, their content is not immediate but secondhand. We cannot point to an objective standard, we can only say that it is our judgment 'that she sang sweetly.' If anyone chooses to differ; that is if his state of mind in the presence of the song was not similar to that on receiving a sweet stimulus, there is nothing to be said. The adverb is not compelling in its nature. It involves a comment on the action of someone else, a comparison of the effect of the action with a similar adjective state. But since it is a comparative comment, it lacks the rich immediate content of the adjective. All adjectives, however, do not express elemental sensitive qualities. 'Quick,' for instance, involves some comparison with other things which might be slower or quicker. Yet even here (with my own subject at least) the same difference was observed in the content which made up the word. Quick was felt in

a sharp tension, without involving any purpose of comparison. It was solely in terms of herself, she was the standard, and there seemed no point in questioning the validity of its use in any phrase. With 'quickly' the scene changed at once to the action of another, that is, part of the experience of the word was to point beyond itself to an action, and since her own action was never felt in terms of qualitative comment, the action was viewed from the outside. There seemed no standard of quickness here. The immediate content of the idea was very thin, as if it reserved itself for some definite standard of comparison. This whole adverb experience was more derived in character than the adjective, felt in terms of comment on someone's else action, with no given standard. It seemed 'far away' in comparison with the adjective, and as if more data in the way of a noun of comparison would have to be given, before it could arouse a complete significance.

Adverbs of place, here, there, etc., were felt solely in terms of tension forward, backward, and so on. These tensions could be expressed by action, but with this difference from prepositions that with the latter there was no purpose and no reference to anything as a standard. They were veritable formulae to be applied to anything; while the adverbs of place, distinctly referred to the self as the point of reference. The preposition expressed an element of relation, where neither thing related was involved. With the adverb one side of the relation is expressed, *i. e.,* the subject feels himself as the point of reference from which the adverb of place takes its direction.

It was suggested earlier in the paper that there is a possible difference between our reaction to a transitive and an intransitive verb. That is, if more is involved in responding to one than to the other, it would show itself in a difference in the rates of introduction into the language. It will be noticed at once, that the very essence of an intransitive verb is that its associations and its object are involved in the word itself. It does not depend on another word to denote the end of its action—viz., we cry, walk, talk, swim, play and every one of these intransitive verbs stands for a complex of motions, with its own end involved in the verb. The verb stands for a complex

of associations, just as a concrete noun does, with the added association of someone being agent in the active process of carrying out the end involved. The verb *walk* means to me the varied association images of the mechanism of walking, someone doing it and the purpose of getting over the ground. The purpose is just as much one of the associations as anything else, and according as we develop new purposes, as we are doing all the time, it is as easy to make a new word to describe the process and the means of attaining this purpose as to make concrete nouns. With the transitive verb the end is not an intrinsic part of the associative complex. The verbs to cut, lift, say, drag, draw, etc., while involving the idea of purposive agency, and having as their content distinct localized motor ideas of our own or someone's relation to something else, do not indicate what the something is, or why we act upon it. With an intransitive verb the purpose, the associations—all that goes to make up the content is involved in the word itself. It requires nothing else to fill out the meaning. There is no object to the verb, because the object or aim of the action is implied in its very essence. The experience is quite stable. With the transitive verb, the tension is localized, and feeling of purpose is there, but what purpose? What is the object of the action? From the very fact that the object is not involved, there is a great possibility of combination of the verb with different objects. But since the verb can take any object, it is by itself devoid of definite content, and there is a void, an expectant pointing beyond to something which is not there, which gives it a somewhat less stable character than the intransitive verb. Such a verb is more abstract in its character than the intransitive verb, though less so than the preposition. That is, the preposition expresses the general attitude—elements with no purposive agency whatever, simply the different possibilities of relation between different things. The transitive verb goes beyond the prepositions in expressing active specialized purpose, but without the object of the purpose stated. It expresses some action or intent with regard to something else, though that something is unexpressed, and there is therefore a void, a slightly unstable character, a potentiality for combination, which leaves

the experience somewhat unfinished. In such a word-experience, to be sure, the observer does supply some object. Almost invariably, some associated image supplies an object for the action. But even then it is felt that the wood, for instance, which fills out the meaning of cut, is not the only possible object. There is a pointing beyond to anything that may be cut, and this pointing gives the unfinished character to the verb. With the intransitive verb, however, all the necessary associations are supplied, there is nothing left to be said. To walk implies something to walk on, the purpose of getting somewhere; indeed, everything which is needed to complete the meaning is there. No void is left for inquiry as to what we walk with, or on, or to, or why we walk at all. Part of the necessary associations are solid substance to walk on, two legs at least, purpose of getting over the ground, etc. The whole word experience is stable and finished, while with 'cut' the very possibility of innumerable objects for the activity, prevents any one from completing the meaning with perfect satisfaction, and the pointing beyond to other possible objects makes the experience less sufficient unto itself.

Concrete nouns, certain adjectives and intransitive verbs seem then to have the most solid self-sufficient basis and are grounded in a cluster of associations, no one of which points beyond, but each supplies a positive content. Transitive verbs share with prepositions the necessity of relation with something else and since this object is unknown in both cases, the pointing to an absent content leaves the experience unstable. This unstable character is, of course, much less evident in the transitive verb than in the preposition, but in the one respect of dependence on an absent object, they resemble each other. Moreover there is a similarity in character between the experience of an abstract noun and an adjective. Both of them possess the adjective characteristic of subjective reaction under the influence of certain qualities, only in the case of the noun this state of mind stands by itself, while in the case of the adjective it reaches out to cling to something else. The adjective cannot stand wholly alone; one of its associations is the pointing out of its application, while the noun has no such out-pointing.

It may be seen from this analysis that certain varieties of words, although classed in a certain way among the parts of speech, bear in their psychological content just as striking a similarity to the experience of other parts of speech, as to the experiences of those parts of speech among which they are classed. It is their use in the sentence that determines this classification ultimately. Simply as word-experiences, standing by themselves, the present classification is not at all adequate to express the shades of difference in our reaction upon them. Moreover, there are all grades of meaning which a word may have according to the purpose of its use at any one time. If the word 'cat' is said alone, it has a broader significance than when used in a sentence where but one cat is indicated. One cannot say that there is a certain breadth of meaning which a word always has regardless of the idea which is expressed.

The different parts of speech vary in this fidelity to their whole significance. A preposition always means all that it can mean, *i. e.*, its meaning consists only in a tension without purpose and where there is no purpose, there can be no variation. Nouns, adjectives, and verbs may have many grades of significance even within one given meaning, according to the purpose of the conversation, and there is much variation of this kind when words in sentences are considered instead of isolated units. With isolated words, however, every word is considered in its broadest sense.

With these considerations in view, what should we expect in the development of language?[1] It would seem that in course of time new concrete objects would come to our experience, therefore, concrete nouns would develop to label them. Moreover, new purposive actions dealing with these objects would arise, in which the end of the action and the appropriate means would be involved. This kind of action is represented by intransitive and especially by cognative verbs. In proportion as life advances in complexity, actions become more complex in their means and in their ends, and to econo-

[1] (The references to treatises on language, histories of its development, experimental investigations of the growth of speech in children, I have grouped for greater convenience, in a bibliography at the end of this paper.)

mize language we invent a verb to stand for the whole complicated action, instead of a phrase (viz., to telegraph instead of to send a telegram). We should, therefore, expect intransitive verbs to develop with greater frequency than transitive. Since the latter are more simple in content and the object of action is not involved, less associations are present, a greater number of combinations are possible, and, being more abstract, their forms would change less easily.

After the intransitive verb has been introduced, apparently the next step is to make it take an object, and so change it from intransitive to transitive. This would arise from the demand of making our own action affect another's—just as we often make intransitive verbs take an object in ordinary conversation, viz., 'I'll walk you.' But it would seem *à priori* that the logical mode of entrance would be through the intransitive verb stage.

Since adjectives express the qualities of objects, we should expect adjectives with specialized reference to external objects, to bear some constant ratio to concrete nouns. That is, as new concrete experiences occur, the objects involved must have the qualities which make the experiences or the objects different from others. The adjectives and nouns must keep pace together. Those adjectives which have the most content in themselves and the least pointing to something else, are the elemental sense qualities or the simpler reactions and judgments upon things. These have a very rich content, and while they still have the function of pointing or belonging to something else, they stand alone so well, that one scarcely feels any difference in the word experience of warm and warmth, blue and blueness. There seems to be no difference except as regards use in a sentence. With adjectives, however, that express the qualities of other things, not at all their effect on us, there is no sensational content whatever. They cannot stand alone as word experiences, they simply serve to point to the noun. Electric, in its strict, not its figurative sense, means nothing to us in itself, it has no content, except as we know its noun. It is an abstraction built out of the noun for the purpose of communication, but has no definite sense content of its own. Since

new concrete objects are always coming to our experience, new adjectives of this type must always attend them. The concrete noun is the name we give the new object; the adjective expresses the quality belonging to or inhering in such a noun. It is not an intimate feeling of meaning, it is simply the concrete noun experience, only instead of the substantial sufficiency of a noun pointing no further than itself, it has the function of referring to just this noun. What new adjectives we find we should expect to be of this description; the adjective method of stating the qualities of a concrete noun. On the other hand, since we have no new elemental sensations and no new attitudes, and our possible reactions on the outside world are limited, we should expect no new adjectives expressing the qualities of abstract nouns. In other words, we felt the adjective quality of blue and warm, of good and bad, etc., before the abstract nouns, which express their essential qualities in noun form. Therefore, the type of adjective which leads to an abstract noun is more full of content, more primitive and elemental in origin and we should expect to change but little. The type of adjective which expresses the qualities of a concrete noun, must be introduced as often as there is a new concrete object in our experience. This points only to the noun, has no primitive elemental content, no effective coloring except as it borrows from the noun, and we should expect it to vary easily.

Reasoning in the same fashion for adverbs, it would seem the change in them on introduction of new forms would be slight. We found that adverbs in general had a derived significance, involving comparative judgment on action and gained their definite content from other words, especially adjectives. We also reasoned that adjectives expressing qualities of abstract nouns would not come into the language as did adjectives from concrete nouns. Now since an adverb made from a concrete adjective cannot have meaning except in a very figurative sense, since many adverbs from abstract quality adjectives can only be used figuratively, since in general, adverbs express comment on action or their place, and these situations do not vary widely, we should expect adverbs to vary but little, less than nouns, verbs or adjectives, since new occasions for their use would be less likely to arise.

Pronouns.—Since their function is to point to any special man, woman, or thing in mind, since no other genders can possibly arise, since no matter how many different particular people or things may be in question at different times, it is the pronoun's function to point to a particular one at any one time—we should expect it never to vary. The fact that they do not vary, and that attempts to introduce a new pronoun (much needed) to express an indefinite reference to a man or woman, have hitherto proved futile, carries out the supposition. If the new pronoun could refer to any particular fixed person, it would be as easy of introduction as a new proper name. We could ask 'Whom do you mean?' and an answer could be given; the pronoun supplied with particular content and after a little use, its place in the language be safe. But the indefinite pronoun, although able to refer to anyone, never can be defined in terms of a particular. To say that at different times, *it* means the chair, table, pen, country, creed or principle, and yet no one of them means *it,* requires a long education in the use of symbols, and the feeling for such a symbol once acquired and the trick of reference learned, we cannot change or adopt a new one. Psychologically, its content is simply the state of mind of pointing to anything we choose, and any such general abstract reference covers the ground so totally that attempts to introduce another tool so versatile are without avail.

The difficulty with the introduction of prepositions was the consideration with which we started. Subsequent analysis of the prepositional states of mind has shown how, expressing as they do the elements of relation with the word in their most abstract character, they should not change since new occasions for them cannot arise and their meaning is so bound up in tensions of the whole organism, that the possibility of variation is reduced to a minimum.

It may be well to emphasize, in closing, that in sentences and all higher language structures, we do not have the complete experience for each word that we do for the same words when spoken separately.

The tendency in all higher word combinations is to temporarily deprive the words of all associations that do not con-

tribute to the meaning of the sentence as a whole. The span of attention is limited, and if we had as complete a reaction as I have described for each word in a sentence, we should be lost in the meaning of the parts before we could combine them in a whole. We do not give equal value to the different parts of a sentence, but we dwell on the more important words, while those of lesser interest serve, not as independent words, but simply as parts of the associative cluster that make up the meanings of the others.

The meaning of a word in a sentence varies, and demands strictly a change in word-form to express this variation, as one or another of its associations or motor reactions is sacrificed for the more concise meaning of the sentence.

The tendency of language development for practical and scientific purposes is all in the direction of economizing the separate word-reactions, for the sake of the meaning of the sentences as wholes. But in these cases, while we get more meaning from the sentence, than we could from any separate word, we are not getting as wide an individual meaning from *any one word* as if we heard it alone. The words limit each other, and by their very definiteness cut off the extent of their individual significance.

The highest literary style consists of a nice adjustment of values, where the words mean all that they possibly can, without confusing the combined meaning of them all. On the other hand, scientific style of expression differs, in that the separate words are allowed only as much independent significance as is necessary for the sentence to have a meaning. This prevents any doubleness of interpretation, and more can be crowded into a given space. In general, a phrase may became a kind of elongated word, and may have meanings in every way analogous to word-units as we have studied them. When these phrase-experiences are described, however, the analysis must fall into the same terms we have used for the word-experiences.

We have chosen to analyze the separate units in the belief that the description of these elements contains in essence the description of all their combinations. In fact, it is only in sepa-

ration that the distinctive framework of the different parts of speech is discernible.

The purpose at the outset of this discussion was to test the conclusions arrived at in this analysis, by actual survey of the tendencies of language development. What information was obtainable on these points bears out strikingly our results. The parts of speech were, of course, more or less arbitrarily fixed by grammarians,[1] and in some languages are not fixed today, but the same syllable varies in this respect according to its position in the sentence. Moreover, the different parts of speech are even now in a loose state, one part interchanged with another according to the demands of the situation. Nevertheless, the parts of speech, as we understand the term, express real differentiations in our feeling for words, and these differences are traced and explained in many histories of language.

I have grouped the authorities which were consulted on the different points in question, under five headings: I., Nature of speech; II., Nature of the different parts of speech; III., Which parts of speech appear first? (a) in the language, (b) in childhood; IV., Which parts of speech vary most easily? V., Aphasic changes.

Under I., no page references were possible, but the whole discussion (especially of Professor Müller) bears out the statements of the necessity of actual reproduction in some way, of the syllable on its own merits, before arriving at the thing symbolized; and of the necessity of speech for thought.

Stout, Romanes and James emphasize the voluntary character of speech, Ribot its evolution from gestures, and the difference between gestures and the true symbolism.

Under II., the page references are given for discussion of the different parts of speech, and in each case the special form indicated. Here the necessity of some general idea in a concrete noun (corresponding to our 'function binding the different images'), is emphasized by Leibnitz and Müller; the peculiar character of prepositions by Adam Smith, Ribot and Brèal, and the universality of the concrete noun, reference

[1] Max Müller, *Science of Language*, p. 102.

character of pronoun, etc., commented on by the other writers. Nouns are discussed with much detail, but the other parts are observed and commented upon more as statements of the problem, than as offering any explanation.

In the third section, the different parts of speech are written under the author's name in the order in which he considers them to have developed. The conformity with our results is almost uniform.

Under (*a*) most writers consider nouns and adjectives to have arisen together at the very beginnings of language, with some priority given to its concrete noun quality, while Ribot considers the adjective quality as primal. In either case according to the example they give, the accord is perfect with the present article. If we consider with Ribot abstract or concrete nouns in their aspect of affecting *us* and bringing about some sensation quality or affective judgment, their meaning would be to us first in terms of adjectives; whereas, if we regard them simply as external objects to which an arbitrary syllable is applied, they would appear in speech first as concrete substantives. The decision depends on the correct theory of language beginnings, but in either case the close union between adjective and noun states of mind is emphasized. Brèal considers that the first syllables applied to objects had a pronoun rather than a noun significance, though his argument does not seem convincing. He agrees with Adam Smith as to the priority of intransitive and impersonal verbs over transitive, while Müller does not give a decisive opinion. It will be observed that in all cases the adverbs come *after* the adjective, and that prepositions appear last of all.

Under III*b* are given results of various observations on the appearance of new words in the vocabulary of a child. In these without exception, nouns appear first and prepositions last, although the order of verbs, adjectives and adverbs of place is not uniform. (A more complete bibliography of this section will be found in Mr. Tracy's article.)

There is a separate section for IV., since slightly different rules obtain in the *order of development* of parts of speech in the vocabulary of the child or race, than hold in their

liability to change. We said that we should expect the parts which came latest, thereby representing a higher degree of abstraction, would be less likely to change than the others. To thoroughly establish this claim, one would have to examine a list of words with their dates of entrance into the language and see if they follow this order. Such an exhaustive study could not be made here, but what testimony we could find in histories of English bears out our supposition. The parts of speech under each author are given in the order of their liability to change, the more variable coming first. Similarly under V., the parts of speech are given in the order in which they are lost; those that disappear first, head the list. Ribot's statement that the particular concrete words disappear first, while the more general words as those expressing relation remain; also that words disappear in aphasia in inverse order to that of their evolution in language, fully accords with our expectation.

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KINÆSTHETIC AND ORGANIC SENSATIONS: THEIR ROLE IN THE REACTIONS OF THE WHITE RAT TO THE MAZE

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PREFACE.

In this place, I wish to state my deep indebtedness to Professor James Rowland Angell for his many kindly criticisms and suggestions both as regards the content and the form of the present paper.

Dr. Harvey Carr has given me great assistance in the conduct of the experimentation. I have tried where possible to indicate the work for which he alone is responsible. During the past summer, 1906, he devoted a large part of his time to coöperating with me upon experiments described in the paper under headings, Part B, IV. and VI.

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JOHN B. WATSON.

CONTENTS.

INTRODUCTION.

PART C.

I. INTRODUCTION.

(*a*) The Nature and the General Bearings of the Present Study.

In a short address given before the Section on Genetic and Comparative Psychology of the Congress of Arts and Sciences, St. Louis, 1904, the writer sketched a possible experimental method in Comparative Psychology, which would have for its goal the determination of the relative importance of the several sensations of any given animal in its adjustment to its environment. As a matter of fact, the method, as outlined there, consists in adapting an established method to meet new conditions.

For many years, physiologists have made use of vivisectional experiments to determine the function of specific organs. Their results have been of immense value both to the physiologist and to the psychologist. On the whole, however, these experiments have usually been made upon untrained animals for the purpose of testing the relatively immediate and local effects of the given operation.[1] Naturally, in such experiments, the physiologist has given little attention to the effect of the operations upon the instinctively and habitually organized reactions of the animal as a whole. Here is, undoubtedly, a new field for the student of animal psychology; a field which must be worked over from the psychophysical standpoint, in contradistinction to the purely physiological one. As is well known, this method has already been applied to the study of defective human beings who have lost or who have never possessed one or several sense-organs, and as a result of the appli-

[1] Shepherd I. Franz's experiments to determine the relation of the frontal lobes to the production and retention of simple sensory-motor habits in cats, *Am. Jour. Physiology,* 1902, Vol. VIII., p. 1 ff., form an exception. His method is practically identical with the one adopted here. Cf. also this author's 'Observations on the Functions of the Association Areas (cerebrum) in Monkeys,' *Jr. Amer. Med. Association,* November 3, 1906, Vol. XLVII., pp. 1464–1467.

cation of this method, a flood of light has been thrown upon the complicated human mental processes as a whole. It is natural to suppose that the study of blind, deaf and anosmic animals may, in a similar way, throw as much light upon the totality of the mental processes of animals. In the case of human subjects, born with defective organs, our interests are mainly concerned with the way in which an organized mind can develop and react upon its environment, when the material upon which it depends for its growth is limited in kind. In the case of human subjects who have suffered either sense organ defects, or defects of the corresponding cortical centers in conjunction with these, after the mental life has become organized, we are interested principally in the reshaping and in the reconstruction of the mental life, as evidenced by the changed behavior of the individual experiencing the defect. Our interests, then, in defective minds are functional; we are interested in the content of such minds only in so far as the analysis of their content will lead to the understanding of the behavior of the subject. So much has been said to show that, from the standpoint of method, the study of minds of animals (normal or defective) does not essentially differ from the study of human defective minds and the minds of children. In view of the fact that in the investigation of the mental life of animals, we are forever barred from studying content (by way of introspection), a certain limited number of psychologists and physiologists are inclined to underestimate the value of the comparative method. The present paper will not attempt to justify comparative psychology; it will take its justification for granted, relying upon the fact that the criticisms directed against the study of the mental processes of animals are valid only from the standpoint of a structural and statical psychology. They have no significance from a functional point of view.

Detailed and careful studies of the sense organ processes of animals will aid us in the end, we feel sure, not only in understanding the organized reactions of the particular species of animal studied, but also in bringing about the possibility of a closer functional comparison of human sense organ processes

with those of animals. The value and general bearings of such studies are well recognized, but the pressing need of such knowledge as a basis for further studies in animal psychology is not so thoroughly emphasized. Elsewhere[1] we have tried to show, that certain problems in animal behavior are, in the writer's opinion at least, not rightly answered today because of the lack of detailed knowledge of the principal avenues over which animals receive their most important sensations. The questions (stating them in conscious terms) are not so much, "Can my animal see? Can he hear? Can he smell?" etc. The problem is, rather, "How well can he see *objects?* Well enough for vision to serve as a specific basis for reacting to them?" "Or does light serve merely as a stimulus (possibly mainly affective) to arouse general activity, while the details of the ensuing adjustment are left by vision to be carried out by other groups of sensations?" It is easy to see that the answer to these questions must make a world of difference in our presentation of problems to him.

In the present paper, we shall deal with some experiments upon the sense organs of the white rat, the results of which we hope will put us in a position to begin a more scientifically based study of his mental processes. These experiments have occupied a large part of the writer's time for a period of eighteen months. The maze (kind, method, etc., to be described separately further on), was chosen as being the problem best adapted to the structure and habits of the white rat. The eminent fitness of the maze to serve as a "rat problem" has already been described by Small.[2] We shall now turn to a study of his experiments upon the rat and to those of other investigators who have observed different animals at work upon this problem.

(*b*) A Summary of the Work Done by Other Investigators upon the Normal Process of Learning the Maze.

As may be surmised from the title of this paper, the present study grew directly out of the repetition of Small's experiments

[1] *Psychological Bulletin,* Vol. III., No. 5.

[2] *Am. Jr. Psy.,* Vol. 12, pp. 206–239.

with rats in the (modified) Hampton Court maze, and it may be said frankly in the beginning, that the outcome of the investigation does not go far beyond the spirit, at least, of Small's interpretations. But we have tried to use tamer animals than he did, and by so doing we have been enabled to improve slightly upon his method. As is well known, Small was the first investigator to show that both the white rat and the wild gray rat can learn the above complicated maze. Small's time records, however, cannot be used as a basis of comparison with those obtained from other animals which have been allowed to form this association. The inaccuracy in his records arises, in the first place, mainly by reason of the fact that he allowed his animals the run of the maze for a whole night before beginning to record the time of their successive trips through the maze. Any one familiar with the habits of the rat knows that 'curiosity' is the key note of his existence. A new situation means the releasing of a great amount of motor energy. This takes the form of the minute examination of all the surrounding territory. An untrained rat at liberty in the maze is not content with a first success: he tastes the food, leaves it and goes out upon a new exploring tour. He promptly gets lost, finds his way back, tastes the food again and again leaves it. This procedure is kept up until both his emotional state is quieted and his appetite appeased. It is clearly evident that if one adopts a method such as the above, one must relinquish the idea of any accurate statement of the early stages of the process of forming this association. In the second place, Small allowed more than one animal to run the maze at the same time. This in itself is destructive of an accurate time record. Even rats living together in the same cage will take 'time out' to smell one another in passing. Small's records are unquestionably quantitatively unusable, but the qualitative side of his work is valuable. His work is so recent in the minds of investigators, that we shall not go into it in any great detail.

Remembering that Small is describing only the latter part of the learning process (since all the animals had had the run of the maze for a whole night unobserved), let us quote his

description of the formation of this association by the white rat.[1] "In appreciating the results of this series of experiments, about the same facts come into view, only more distinctly, as in the case of the wild gray rats; the initial definiteness of movement and the fortuitousness of success; the just observable profit from the first experiences; the gradually increasing certainty of knowledge indicated by increase of speed and definiteness; and the recognition of critical points indicated by hesitation and indecision; the lack of imitation and the improbability of following by scent; the outbreak of the instincts of play and curiosity after the edge of the appetite is dulled. In addition are to be noted the further observations upon the contrast between the slow and cautious entrance into, and the rapid exit from the blind alleys, after the first few trials; the appearance of disgust on reaching the end of a blind alley" (Small indulges in a little anthropomorphism here); "the clear indication of centrally excited sensations (images) of some kind; memory (as I have used the term); the persistence of certain errors, and the almost automatic character of the movements in the later experiments. Viewed objectively, these observations all converge towards one central consideration; the continuous and rapid movements of the rats in threading the maze amounting to almost perfect accuracy in the last experiments. . . ."

Since we are, at the present moment, concerned with the establishment of the norm of the process of learning the maze as a whole, we shall leave the discussion of Small's analysis of the sensory factors entering into the formation of this association to a later place in this report.

Kinnaman's observations of the behavior of two Rhesus monkeys in the maze should next be considered.[2] He constructed a large maze, built on exactly the same plan as the one used by Small with the rats. The dimensions of this maze were as follows: 17 feet long, 13 feet wide, and 14 inches in height. The width of the alleys was one foot. The material used in constructing the maze was common chicken wire. The

[1] *Ibid.*, p. 218 f.

[2] *Am. Jour. Psy.*, Vol. 13, I., 98–148; II., 173–218.

learning process here as with the rats was a gradual one—a good deal more so than that of the rats. Kinnaman arbitrarily decided to regard the maze as learned, when the animals could make ten successive trips without error. In general, the behavior of the monkeys in the maze was like that of the rats; the persistency of certain errors, the rapid elimination of certain others, the increase in speed after the first few successful trials, etc., were all observed. A marked individual (or sexual?) difference was found to exist between the male and the female monkey. In the 13th trip, the female went without error; whereas the male did not parallel this until his 36th trip. The female reached the above arbitrary standard (10 successive errorless trials) of excellence on her 66th trial—the male on his 113th. His movements were faster than hers; he attained to an average time of 44.8 seconds, while the female average never went below 55 seconds.

Kinnaman[1] tries to compare the rate of improvement of the successive trials of the monkeys with that shown by one of Small's rats, but for reasons stated above, the comparison is almost valueless. As far as the comparison of the two time records is at all possible, it shows that the percentage rate of improvement in the two cases is somewhat similar. A more accurate comparison of the absolute rate of improvement of monkey and rat can now be made by taking the results obtained from our own combined records (see page 23). On page 185 of the article referred to above, Kinnaman has given a tabulated report of the maze results of the monkeys. He throws the consecutive trials of the monkeys, separately for the male and the female, into eleven groups of ten and takes the average of each group. For convenience of comparison, we will present the first three group averages of this table (changing seconds into minutes), and throw our own results obtained from the white rat into a similar table. The comparison can be carried out only to the 30th trial, since the rats were not given more than that number.

[1] *Ibid.*, pp. 185–186.

	1st ten.	2d ten.	3d ten.
Male Monkey	12.10	1.22	1.26 Min.
Female Monkey	23.35[1]	2.12	1.06 Min.
Rats	4.49	.68	.42 Min.

On page 186 of the same article, he gives the individual time records for the first ten consecutive trials of the male and of the female monkey (from which the above group average of ten was made). Below, we reproduce these, again changing seconds into minutes, and give for comparison the first ten consecutive trials of the white rat:

1st.	2d.	3d.	4th.	5th.	6th.	7th.	8th.	9th.	10th.
45	30	7	3	15	3.8	6.5	4.7	2.6	3.4 Min.
55	132	14	7	6.2	7.5	3.5	3.2	2.7	2.2 Min.
16.20	7.21	7.01	2.90	2.9	2.65	2.09	1.23	1.65	1.06 Min.

Taking the two sets of records at their face value, the monkey isn't 'in it' when it comes to a race in learning the maze. But we must remember first of all, that Kinnaman's records are based upon two animals. We have found several rats from time to time which could not learn the maze any faster than these monkeys. Under more suitable conditions and with a larger number of animals, the record of the monkey might be materially lowered. In the second place, before any comparison can be made, we ought to know something about the respective speed of the two animals in traversing a labyrinth path. Kinnaman's maze was larger than our own. He gives the length of the true pathway as being 105 feet. The final minimum time for traversing this distance was, as has been stated, 44.8 seconds for the male monkey, 55 seconds for the female. This would make the male's speed in the maze (if it were uniform) about 2.3 feet per second, the female's, 1.9

[1] Attention is called to an error on Kinnaman's part in obtaining the *average time of the first ten trials* of his female monkeys. On page 185 of the article referred to he gives this average as being 2,579 seconds. If one refers to page 183 one finds that the times of the first ten individual trials which were averaged read as follows:

3,300	7,920	840	420	375	450	210	195	165	135 sec.

The average is 1,401 sec., or almost one-half the time as given by Kinnaman. This error serves to vitiate all the percentage values of the female's record, etc., which were made upon the basis of the average of her first ten trials.

feet per second. The length of the true pathway of our maze is forty feet. The final minimum time consumed by the rat in traversing this distance at the end of 30 trials is on the average about 20 seconds (many of our rats after having 50 trials can run the maze in ten seconds. The minimum record in the laboratory is nine seconds). This makes his speed in the maze about 2 feet per second. If Kinnaman's times for the monkeys are at all representative, it would appear that the final rate of the monkey is about the equal of the rat's at his 30th trial.

Porter[1] has made similar experiments upon the English sparrow. The learning process with the sparrow is similar to that of Small's rats and Kinnaman's monkeys. Porter makes a detailed comparison of the rate of learning between the monkeys and the sparrows.[2] He likewise throws the consecutive trials of one female sparrow into groups of ten and averages them. These averages of the group records are given below (in minutes).

	1st ten.	2d ten.	3d ten.	4th ten.	5th ten.	6th ten.
English sparrow	8.2	6.2	1.8	1.2	.92	.56 Min.

Porter gives separately the record of the first ten consecutive trials of two females and one male. We have ventured to average these, thinking that we should obtain a more typical record thereby. This average record of the first ten consecutive trials of the sparrows follows immediately:

1st.	2d.	3d.	4th.	5th.	6th.	7th.	8th.	9th.	10th.
36.1	19.2	11	8.4	2.6	3.1	4.3	2.3	5.1	6.6 Min.

Porter states that his maze was exactly one-half the size of Small's. The true pathway of Small's maze measures approximately 50 feet (so Professor Sanford kindly tells me), that of the Porter maze then should measure 25 feet. The sparrow's final minimum time of travelling this distance is 33.7 seconds, consequently his speed in the maze is approximately 1.3 feet per second. If we make allowances for this difference in speed which is probably due to structural causes,

[1] *Am. Jr. Psy.*, Vol. 10, pp. 313–346.

[2] P. 339 ff.

the sparrow's minimum time is proportionately equal to that of the rat. (If the distance were made proportional to the speed, the length of the true pathway of the rat's maze should be 38 feet which is almost the length of our own maze—$1.3:25::2:x$, so $x=38$ feet). It is rather singular that the two mazes should have been constructed so nearly proportional to the respective speeds of the two animals. If we turn to the absolute rate of learning of the two animals (cf. first ten trials of sparrows and rats) we notice that the sparrow's time records are very much inferior to those of the rat. But again, we must bear in mind that the number of sparrow records like those of the monkeys is too limited to enable us to draw any kind of safe conclusion.

The maze in its simpler forms has been presented to several different kinds of animals—Yerkes[1] experimented upon the green frog, Yerkes and Huggins[2] upon the crawfish, Triplett[3] upon the perch, Watson[4] upon the white rat, Allen[5] upon the guinea pig, Porter[6] upon the English sparrow and other birds, Rouse[7] upon the domestic pigeon, Fielde[8] upon the ant, etc. In so far as the above-mentioned investigators contribute anything towards the possibility of the analysis of the sensory factors entering into this association, they will be discussed in detail under Part B, a, page 25. Many interesting facts bearing upon the learning of the maze as a whole have been brought out by these comparative tests upon the different animals; such as the degree of complexity of the maze which a given animal can learn; the absolute and relative time records; the percentage rates of improvement, etc., but the separate consideration of these facts would take us too far afield.

[1] *Harvard Psychological Studies,* I., p. 579.
[2] *Harvard Psychological Studies,* I., p. 565.
[3] *Amer. Jour. Psychol.,* 1901, XII., pp. 354–360.
[4] *Animal Education,* p. 59 ff.
[5] *Jour. Comp. Neurol. and Psychol.,* Vol. XIV., 1904, pp. 293–359.
[6] *Amer. Jour. Psychol.,* Vol. XVII., No. 2, pp. 246–271.
[7] *Harvard Psychological Studies,* II., pp. 581–613.
[8] *Proceedings of the Academy of Natural Sciences of Philadelphia,* 1901, pp. 521–544.

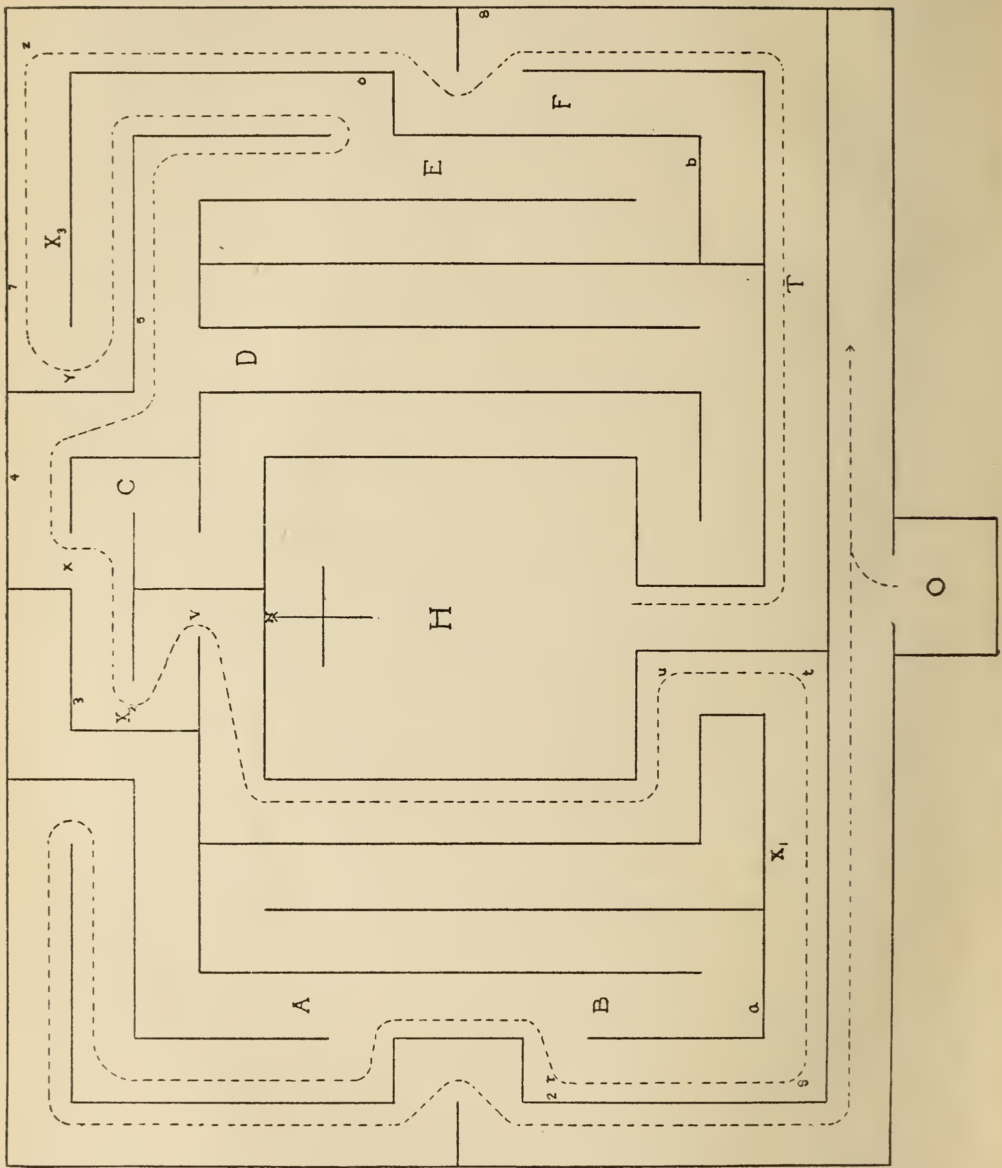


FIG. 1. Plan of maze used in all the following experiments. (Adapted from Small.)

(c) The Description of the Maze, and the Method of Experimentation Used in the Present Investigation.

The same maze was used in all the experiments which follow. The bottom of the maze and the sides of the galleries were made of wood ⅞ of an inch thick. The top of the maze was of wire netting, ¼ inch mesh. The plan of the maze was a duplicate of the one used by Small in his investigation at Clark University,[1] except that the dimensions of the ground plan of his maze were 6 by 8 feet, while those of ours were 5 by 7 feet. The dimensions of the galleries in our maze, however, were exactly the same as in his. This means, of course, that our central food-box was smaller than that of the maze used at Clark. The distance from the entrance of the maze to the central food-box was 40 feet. This distance is represented in Small's diagram by the broken line. It is then a measurement of the approximate route which the rat is forced to take in rounding corners, etc. As before stated the actual measurement of this distance on the maze at Clark is about 50 feet. The true pathway in our maze is shorter then than that of Small's maze by 10 feet. Since our rats, when trained to the maze, covered this distance in about 20 seconds (at the end of 30 trials), their rate of movement is about 2 feet per second. This makes it possible, by adding 5 seconds to any of our individual time records to make them absolutely comparable with those obtained by some other investigator observing the behavior of rats in a maze of the exact dimensions of Small's.

In front of the entrance to the maze, *O* in our diagram, a box 8 inches wide, 11 inches long and 4 inches deep was nailed to the maze. This was covered by a hinged wire top. This box afforded a convenient way for admitting the rat to the maze.

The main difference between the two mazes lies in the materials used in their construction. The one adopted by Small was made throughout of wire netting. This permits an animal to see from one gallery to the next. In our maze,

[1] *Am. Jr. Psychology,* Vol. 12, p. 207 ff.

this was impossible. What influence this fact may have upon keen-sighted animals is not known: That it makes little or no difference in the case of the rat, the sequel will abundantly show.

The method of conducting the experiments here reported (and this applies to Dr. Carr's experiments as well as to our own), was as follows: A quiet time of the day was chosen for conducting the work—9 A. M.–12 M. in our own series, 4 P. M.–6 P. M. in Dr. Carr's. Food was placed in the central food-box. This food consisted usually of a saucer of milk-soaked bread. By the side of this dish, a small piece of cream cheese was placed (the higher the cheese the better!). A large piece of fresh-toasted cheese was placed on the outside of that part of the wire netting which served as a covering for the maze, immediately over the center of the food-box. This larger mass of cheese served the purpose of increasing the intensity of the stimulus.

The particular animal whose record was desired was placed in the little box leading to the entrance of the maze, and enclosed there. He would usually smell the floor and corners of this box for a time (even though trained to the maze) before passing into the gallery. His time was taken with a stop-watch from the moment he entered the maze until the food was reached. He was allowed to eat of the food for a short time[1] after which he was taken out, stroked for a few minutes, then tried again. Usually four trials were given in this way. Sometimes when the animals reacted well, five trials were given (rarely six). On the other hand, if the animals were not in good condition (even when every care is taken as regards the amount of food given the day before, etc., this will occur) only one reaction was taken. A diary record, showing the number of trials taken each day, the exact number of hours elapsing between any two sets of trials, the number of errors, etc., was made. These minute records, however, are not reproduced in the tables shown in this paper. In our own experiments the rats were observed *every day*. If this was not done, the animal was likely to get out of the 'swing,' and his

[1] See Watson, *Animal Education*, p. 9.

first time record after a day or two without experimentation was likely to be too long.

After the animal had finished his set of reactions for the day, he was allowed to eat freely of the milk-soaked bread. When he showed signs of satiety, which he manifested by beginning to 'explore,' he was removed to his living cage and fed no more until tested in the maze on the following day. Utter hunger was thus avoided. Our rats, on the whole, have been in *better physical condition* during the period of experimentation than when they were allowed to live quietly in their cages. This has been emphasized, much to our sorrow, as we write the present paper. For two months, our rats blind, anosmic and normal have been idle. At the present moment, all of them are in poor shape. When we finished our experiments upon them, they were in perfect condition. Much has been writen about the artificiality, the abnormality—yes, even the brutality of the present 'laboratory' method in animal psychology. However well founded they may be in certain cases, these criticisms cannot with justice be urged against the present set of tests.

In our experiments upon the 'normal control series,' a cumulative stop-watch was used. When an animal stopped to scratch, or to bite at some particle in the maze, the time used in the process was taken out. This time, however, was found to be insignificant after the first few trials of the rat. When we found that the records taken on the blind and on the anosmic rats were likely to be better than those of the normal control, we abandoned the use of the cumulative watch, for fear some one might think we had unconsciously (!) helped the animals' records a trifle. All of Dr. Carr's experiments were timed by an ordinary continuous stop-watch.

There is one serious defect in the Small maze. We find it impossible to take an accurate account of the errors in it. As a matter of fact, we have recorded the errors made by our rats in the way suggested by Small, Kinnaman and Porter. They fill one or two note-books, but we feel sure that they are not worth the time we spent in recording them—certainly are

not valuable enough nor accurate enough to publish. Since this position makes the absolute time record the only criterion of the learning process, we have used extra precautions to make it show what it is meant to show, viz., the relative rapidity with which normal and defective rats form the maze association. We have both time and error records before us, and we unhesitatingly say, that the time record, carefully controlled, is the only safe guide in estimating the learning process of a maze constructed along the lines of the present one.

In these experiments on the normal and blind rats, no effort was made to rule out the help which smell might give the untrained rat in the form of *tracking*. Small covered the bottom of his maze with fresh sawdust, and changed it each time a new rat was introduced to the maze. But since Small concludes that smell in the sense of tracking plays little or no part in the formation of this association; and since this conclusion is in agreement with the results of some experiments which we made upon the white rat some years ago,[1] we decided to neglect this possible source of error. In view of the fact that our previous work shows that the element of tracking is possibly present when males and females are both traversing the same pathways, male rats were used in most of the experiments in the present series. The two anosmic females observed are exceptions, but here, fortunately, since smell is ruled out, the exception makes no difference. However, their records are shown separately.

In concluding this section, it remains to be said, that all of the animals used in this work were exceedingly tame. In addition to the fact that they were pets to start with, they were fed in the food-box of the maze each day for a week before they were allowed to learn the maze. They were, of course, strictly confined to the food-box, and were not allowed to traverse any other part of the maze. This causes the animal to become accustomed, (1) to obtaining his food in the maze, (2) to the noises necessary in opening and closing the maze, and, (3) to handling. The benefits accruing from this method were not

[1] *Animal Education*, p. 53.

fully appreciated until after the normal control series had been taken. The animals used for obtaining this record had not received the proper amount of this preliminary experience before their reactions were taken. Undoubtedly, this makes their time averages for the first few trials too high. Fortunately, the 'combined' record and Dr. Carr's records control the defects in our own.

PART A.

The Results of Experiments Upon the Behavior of Normal Rats in Learning the Maze.

(*a*) *Experiments Upon Four Rats to Establish a Normal Control Series.*

Since the main facts in the present paper are concerned with the comparison of the behavior of the normal rats in learning the maze, with that of animals having defective sense-organs in forming the same association, the first step to be taken is to establish a normal record of the learning process. This is all the more imperative, since, as we have mentioned above, Small's time records are unusable. But since Small has so adequately and so minutely described the qualitative features of the behavior of the gray and white rats in their later reactions to the maze, we shall pass over the description of the details of the formation of this association, saying a few specific words, however, about the early stages of the learning process.

In the fall of 1905, four male rats about one year of age were started upon the problem of learning the maze mentioned above.

In general, the behavior of these rats in the maze may be described by the following statements: The first few trials in the maze are characterized by the making of every possible error. The food at first apparently exerts little or no influence in drawing the animal to the food-box. The stimulus of the new surroundings is more potent and the animal's 'attention' roves freely from one part of the maze to the other. This type of behavior stands out in bold relief against that of the fully trained animal. In the latter case, the food is the emotionally exciting object. It compels his attention from the start and retains its power to the end.

One very peculiar type of error was made by all of our rats in the beginning of the experiments, viz., that of 'back-

tracking.' The rat first becomes emotionally neutral to his starting point, *i. e.,* the small box at the entrance to the maze, to which he always has access. His first movements away from this neutral position do not carry him any great distance. He then returns to the starting point—his second advance usually carries him farther than his first. This process of advancing and retreating, making errors all the time, ends finally in his *learning* the maze *backwards* by the time he has learned it forwards.[1] We emphasize this point especially because many have thought it strange that the rat could get back from the center of the maze without ever having had his food stimulus at the opening to the maze, and his starting point at the center. We have seen our rats make several errors in going towards the food-box; getting almost there, they would run into the last possible cul-de-sac, come out, take the 'back-track' and come rapidly home without an error. The causes for this behavior are to be sought for undoubtedly in the emotional condition of the animal. While it is not true of the very young animals, it is true of these four adult males, that the maze undoubtedly stirred the emotion of fear in them. This was especially true of one rat—the one which gave the maximal time in Table I. (p. 19). His behavior was so marked that it deserves mention. He hugged the sides of the galleries and both advanced and retreated by furtive dashes. This was true, notwithstanding the fact that he was perfectly tame when held in the hands. We feel sure that the stimulus to the learning of the pathway back to the entrance lies in the 'at home' or 'of course' mood which, being produced there, brings relief from emotional excitement. (It must be remembered that our rats, once reaching the food-box, were not allowed to run back. They were allowed to get to the food-box the best way they could, by running backward or forward, etc., but once there, they were restrained from leaving it until a new trial was started.)

After the first success in the maze, the absolute *time* of the

[1] This is not quite true! Dr. Carr made a series of tests upon trained animals by putting the food at *O* and starting the rat at *H*. It took the rats 2½ trips on the average to become perfect after this reversal of the pathway.

succeeding trials is often reduced before the number of errors is reduced. The animal is more active because the food has become a specific stimulus. This state of affairs may continue for several trials. The first signs of the decrease in the number of errors are to be found when the animal ceases to run the full length of the cul-de-sacs—he makes the initial error, *i. e.,* by leaving the true pathway, but he no longer follows his error out to the bitter end. He will often turn in the middle of a cul-de-sac and retrace his steps to the true pathway. (This is the stage marked by Small, when he says they turn showing apparent signs of disgust.)

The second stage in the process of decreasing errors, is marked by the animal's showing simple *hesitancy* at the turns. As long as the track is straightaway, there is 'full steam ahead'; the moment a turn is reached, a slowing of the speed of the animal is noticeable. Sometimes his momentum actually carries him beyond the correct turn. A full stop is then made, and the animal retraces his steps and turns into the true pathway. As is expressly stated by Small, Kinnaman, and Porter, the various possibilities of error are not equalized; some errors are persistently adhered to, while others are rapidly overcome.

From this point on, the hesitancies become of less and less duration, and they are made at fewer and fewer places. Finally the animal becomes a veritable automaton. At this stage, the time becomes practically constant and is reduced almost to the actual reaction-time of the animal in traversing a tortuous pathway, 40 feet in length.

Table I. and the curve constructed from it (Curve I.) show the details of the average times of the first and succeeding trials of four normal rats in the maze. Table I. shows in addition the maximum and minimum times at any given trial. The maximum and minimum times are individual records, of course. They are not always taken from the same rats in the various trials. It may very well happen, and often did happen, that the *maximum* time on the 7th trial, for example, was made by the rat which gave the *minimum* on the 5th trial.

TABLE I. Showing the *average*, the *minimum* and the *maximum* times of four adult male rats in learning the maze. (Normal control series.)

No. of Trial.	Average. Minutes.	Minimum. Minutes.	Maximum. Minutes.
1	29.01	6.28	87.41
2	10.59	5.56	23.46
3	12.31	2.31	31.31
4	3.80	2.46	5.44
5	4.42	2.33	8.70
6	5.01	2.21	5.44
7	5.15	1.75	8 21
8	2.12	.65	5 30
9	2.24	1.00	5.05
10	2.09	1.06	3.08
11	1.86	41	4 71
12	1.60	.32	3.76
13	1.17	.33	2.98
14	1.65	1.20	2.63
15	1.08	.36	1.98
16	.63	.25	1.23
17	2.86	.68	8.46
18	1.06	.55	2.38
19	.76	.35	1.48
20	.95	.48	1.55
21	.42	.28	.55
22	.79	.53	1.45
23	.67	.43	.91
24	.68	.30	1.41
25	.57	.30	.90
26	.55	.28	.91
27	.72	.35	1.18
28	.49	.25	.96
29	.56	.25	.86
30	.45	.23	.65
31	.51	.23	.98
32	.46	.31	.78
33	.38	.31	.41
34	.38	.23	.58
35	.42	.25	.78
36	.31	.20	.45
37	.29	.23	.33
38	.35	.25	.43
39	.44	.28	.81
40	.30	.26	.38
41	.31	.23	.40
42	.41	.25	.61
43	.38	.23	.55
44	.37	.22	.55
45	.31	.21	.51
46	.27	.23	.33
47	.26	.23	.33
48	.26	.20	.38
49	.28	.20	.46
50	.30	.18	.50

A separate and detailed discussion of this table and its accompanying graph is not necessary. It shows the relatively long time of the first trial and the rapid decrease in the time

of the succeeding trials—the time of the 19th trial falling below one minute and remaining lower than that for all the succeeding trials. This time record of the normal rats in the maze is by no means an unusually low one—in fact, from the experiments of our students and later ones of our own, we are inclined to think that it is somewhat too high to be really representative. We have already stated, however, that these rats

CURVE I. Showing the normal process of learning the maze. Based upon four normal male rats, about one year of age. In all of the following curves one division of the ordinate represents one minute, while one division of the abscissa represents one trial.

were one year old, and that they were more timid in the maze than any of the other rats with which we have experimented. The possible influence of the age of the animal upon the time of the formation of the maze association will be further discussed in the section immediately following.

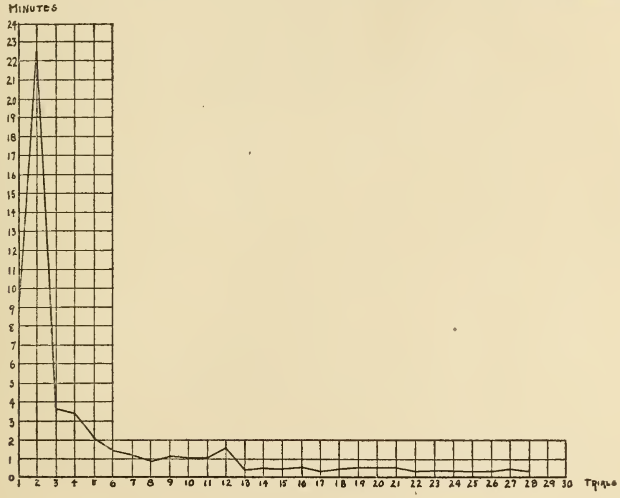
(*b*) *Preliminary* (*Unpublished*) *Experiments Made by Dr. Harvey Carr.*

Dr. Harvey Carr, working in this laboratory in the spring of 1905, observed a number of very young rats in the act of

forming this association. The record of his experiments on three of these rats (35 days of age at the beginning of the experiment) are tabulated in Table II. and are shown graphically in Curve II.

TABLE II. Showing Dr. Carr's experiments on the learning of the maze by three very young rats.

No. of Trial.	Average. Minutes.	No. of Trial.	Average. Minutes.
1	9.76	15	.44
2	22.50	16	.53
3	3.70	17	.33
4	3.46	18	.45
5	2.08	19	.48
6	1.58	20	.48
7	1.30	21	.49
8	.99	22	.26
9	1.15	23	.33
10	1.05	24	.30
11	1.02	25	.33
12	1.55	26	.35
13	.41	27	.50
14	.51	28	.30



CURVE II. Showing the normal process of three young rats in learning the maze. The rats were about 35 days of age at the beginning of the experiment.

The above is undoubtedly a very remarkable record. If one compares it with our own just reported, one cannot fail to be struck by the fact of the greater rapidity of the learning process of the young rats. After the second trial, the average

time drops below 4 minutes and constantly grows smaller, until on the 13th trial the average falls below one minute (the 19th trial marks the corresponding point in our own series). In addition to this more rapid shortening of the time in the first few trials, we notice that the average of the last five trials, from the 24th–28th inclusive (Dr. Carr's records extend only to the 28th trial) is .37 min., while this average is not reached by our rats until the five trials lying between the 33d–37th. Although the number of animals in the two cases is too limited for any generalization, we feel sure that the apparent differences, as shown by the above records, between the rapidity of the learning process in the young and in the old rats would be confirmed by a more extended series of experiments. The factor of age is undoubtedly of importance whenever a comparison of any two records is desirable.[1]

(*c*) *A Combined Record of the Time of the Learning of the Maze* (*Based Upon Nineteen Normal Rats*).

In view of the fact that most of the curves illustrating the formation of animal associations have been based upon a very limited number of animals, and inasmuch as a certain theoretical interest attaches to the form of such curves, we have ventured to construct a table and curve (Table III., Curve III.) which will show the average time of 19 rats upon their 1st, 2d and succeeding trials in the maze, the average mean *variation* in time at each trial; and finally, the maximal and the minimal times at each trial. The separate records going into this table are taken from the writer's experiments, Dr. Carr's, Miss Richardson's and Mr. Peterson's (Fellows in the Department of Psychology). The data are not ideal by any means. In the first place, they are taken from rats of different ages; from rats not all of which have had the same previous experience with other problems; from three rats which were given *one* trial each day, whereas all the other rats were given 3, 4 or 5 trials each day; and finally, from both male and female rats. But notwithstanding this apparent heterogeneity in the sources of the data, we feel sure that the *form* of the curve is

[1] Cf. *Animal Education*, p. 84.

not altered by these differences—the effect of ideal conditions would probably be felt only in the lowering of the absolute time of any given trial.

TABLE III. (Curve III. is inserted at end of paper.) Showing average time, average mean variation, maximal and minimal time, and percentage decrease in time of 1st, 2d and succeeding trials. (Based upon 19 normal rats.)

No. of Trials.	Average. Minutes.	Mean Variation. Minutes.	Maximal. Minutes.	Minimal. Minutes.	Percentage.
1	16.20	12.08	87.41	1.36	100.0
2	7.21	5.69	40.00	.93	44.5
3	7.01	5.63	31.31	.41	43.2
4	2.90	1.34	6.00	.25	17.9
5	2.90	2.15	9.00	.28	17.9
6	2.65	2.11	10.41	.31	16.3
7	2.09	1.43	8.21	.33	12.9
8	1.23	.80	5.30	.23	7.5
9	1.65	1.26	5.41	.33	10.2
10	1.06	.61	3.08	.21	6.4
11	.90	.59	4.71	.31	5.5
12	.90	.66	3.76	.21	5.5
13	.67	.44	2.98	.25	4.1
14	.84	.59	2.73	.16	5.1
15	.64	.34	1.98	.18	3.9
16	.46	.23	1.20	.22	2.8
17	.51	.24	1.58	.17	3.1
18	.59	.29	2.38	.18	3.6
19	.59	.32	1.48	.20	3.6
20	.70	.40	1.95	.17	4.3
21	.51	.29	1.83	.15	3.1
22	.43	.20	1.45	.17	2.6
23	.48	.30	2.00	.16	2.9
24	.48	.26	1.41	.18	2.9
25	.39	.18	.90	.16	2.4
26	.41	.20	.91	.19	2.5
27	.46	.23	1.18	.16	2.8
28	.35	.14	.96	.17	2.1
29	.35	.14	.86	.18	2.1
30	.39	.12	.65	.18	2.4

The form of this combined curve has theoretical interest in view of Hobhouse's criticism of the conclusions drawn by Thorndike from the curves illustrating the learning processes of his dogs, cats, chicks and monkeys. Thorndike states that the method of learning in these animals is a gradual one of 'trial and error'—corresponding, therefore, more or less closely to Hobhouse's method of learning by assimilation. Hobhouse states that Thorndike's curves do not show a gradual learning process—'unless a steeple tower' can illustrate such a method. In view of the nature of the maze problem, and the rats' method of learning it, viz., without the aid of any

extra-organic sensory data (the justification for this statement will be found in our 'conclusions'), it is hard for us to see how the learning process could be any other than that of trial and error, or that of 'assimilation.' Indeed, we cannot believe that even a human being in the possession of all his thinking faculties, if forced to learn the maze under conditions identical with those maintained for the rat, could learn it in any other way than by the slow acquisition (it would be exceedingly slow at that) of a motor habit. And yet, if we turn to our combined curve for the rats, we see that it, like Thorndike's, possesses the 'steeple tower' appearance; but to our minds, this does not militate against the view that the rats' method of learning this problem may be a 'gradual' one. We would account for the 'steeple tower' appearance of our curve by appealing, 1st, to the facts concerning the emotional condition of the rats in their first few trials in the maze, and 2d, to the lack there, at first, of a specific and controlling stimulus. The first point needs no elaboration. In regard to the second point, we have in mind the fact that in the beginning of the learning process, the maze as a whole offers no such stimulus as it does later, after the animal has 'learned' that food is at the 'other end of the line.' On the contrary, the maze offers at first what, for lack of a better term, we may call 'part stimuli': The rat's 'attention' is distracted by the various odors, contacts, etc., at every corner of the maze. The learning process as a whole may be looked upon as the *establishment of a stimulus;* and, as far as we can see, this process of establishing the maze as a specific stimulus *need not necessarily be a gradual one*—*i. e.,* the first two or three trials might be most effective in overcoming the initial neuro-muscular inertia. This might be true, and still the mental process involved in the learning of the problem might not rise above the level of the 'method of assimilation.' On the whole, we feel that Hobhouse's position with reference to the form of Thorndike's curves is not well taken.[1]

[1] We realize, however, that the above statements of ours are summary and more or less dogmatic. Our defense lies in the fact that the complete theoretical discussion of the form of the curve necessary to illustrate the trial and error method of learning would require a paper devoted to that topic alone.

PART B.

Attempted Experimental Analysis of the Sensory Factors Entering into the Formation of the Maze Association.

I. *Historical Survey and Report of Some Unpublished Preliminary Experiments.*

(*a*) *Review of Results Obtained by Other Investigators.*

Small, it is of interest to note, although one of the pioneers on the maze problem, is the investigator who has gone farthest in the attempt to show experimentally the number and the complexity of the sensation processes involved in this form of association. Kinnaman and Porter, both of whom used a maze built on the plan of Small's, seem to have been interested mainly in the fact that their animals had the ability to form this association. Kinnaman does not state whether his monkeys could run the maze in the dark after having previously learned it in the light. Porter would have had difficulty in making his sparrows work in darkness, but the monkeys very probably would have had no such scruples.

Turning to the details of Small's investigation, we find[1] him making the following statements: "The sense of smell might be supposed *a priori* to play the leading rôle, but in the present case its claims to priority are doubtful. In the preceding section, it has been shown that the location of the food by odor, and hence the end to be reached, was an important factor. . . . In general, animals perceive direction of odors only with the aid of air currents. The perception is quite as much tactual as olfactory. It is even clearer that the trail of the first accidental success was not followed subsequently by scent. In the first trial, the rats invariably traversed practically all of the galleries, and, after appeasing their hunger a little,

[1] *Am. Jr. Psy.*, Vol. 12, p. 232.

carefully investigated the entire maze. It would be impossible, therefore, for them to select the right path by scenting the trail. Again, the second rat frequently turned aside from the route marked out by his immediate predecessor. . . . Further, the recognition of critical points, and the fact that the rats frequently ran long distances with heads up—*e. g.*, when carrying food—are evidence against this supposition. . . .

"Another possibility in regard to smell is that particular points in the maze may have been associated with definite peculiarities of odor. The constant sniffing and extensive olfactory investigations of the rats lend color to this thought. The experience thus acquired may, however, influence only the affective tone—connect directly with the emotional tendencies which determine the animal's conduct. . . . The inference is clear that the effect of smell sensation is general and emotional, rather than that delicate and discrete associations of odors with special positions are set up. The point is, however, not absolutely secure. Probably more conclusive evidence might be obtained by testing rats with olfactory nerves paralyzed."

These sentences just quoted from Small's paper are undoubtedly very accurate statements of the inferences one must draw in observing the behavior of normal rats in the maze.

Having (inferentially) ruled out smell as a *cognitive* factor in the formation of this association, Small summarizes the results of his experiments upon vision. We will again quote from the articles referred to:[1] "As the rats did most of their exploring in the dark (*see criticism of Small's method above*), and as the brightness element is only one factor in the visual datum, not the total datum as with the insect, it was improbable that this factor (*of the direction of the light*) (italics ours) should be very influential.[2] Nevertheless, it was made as a matter of experiment. Tests were made by having the

[1] *Ibid.*, p. 234.

[2] If Small means to imply here that the rat discriminates colors as well as brightness, he is using his imagination! There are no experiments discussed in the literature of this subject which show that the rat reacts to colors apart from their brightness. For six months, Mr. and Mrs. Hayes (whose results, it is hoped, will shortly appear), working in this laboratory, have attempted to get unequivocal results upon this very point.

rats learn the path perfectly with the direction of the light constant. The light was then transferred to the opposite side for a few trials, after which it was alternated at unequal, though frequent intervals. The results were: (1) In most cases, change of the direction of the light seemed to produce a very slight effect upon celerity and certainty of movement, but hardly more than might occur as normal variation under constant conditions. (2) Some subjects showed absolutely no effect. (3) After the first change, the alternation produced no effect."

However much we may be inclined to accept Small's bare statement on this particular point of the influence of the direction of the rays of light, we should have been indebted to him if he had given us a record: (1) of the number of rats used; (2) of the time of the trials immediately preceding the change in the direction of light; (3) of the time of the trials immediately subsequent to the change. As a matter of criticism, we may say that the method of learning in the first place—that of allowing the rats to run the maze all night without record of their movements—is wholly prejudicial to any later experimentation in the light.

A second set of tests was devised by Small to find the part played by vision in the reactions of the rat to the maze at all critical points.[1] "Bright red posts, one-fourth inch in diameter, were placed in the middle of the right path a few inches beyond the dividing of the ways. When the rats had learned the path perfectly, the posts were removed. Two rats were tried, the results being *nil*. These rats did not learn the path more quickly; nor did they exhibit the slightest variation in conduct after the posts were removed."

The criticisms upon this method are obvious: In all probability, the rats react mainly to gross changes in *brightness*. Reactions to colored reflected light, under ordinary conditions, at least, are of the vaguest kind—*if they exist at all*. This statement as it stands is dogmatic but it is again based upon the results of the observations made by Mr. and Mrs. Hayes.

[1] *Ibid.*, p. 234.

Still another group of experiments was made by Small upon blind rats. These experiments and the results from them will be stated in Small's own words:[1] "A number of my rats came to me with diseased eyes. Before I discovered this, two of them, an adult male *X*, and a young female (about ten weeks old) *Y*, had become blind. I had already started them learning the maze, with two others, when I noticed their blindness. After the fifth experiment, they were totally blind. In the first two experiments, distinct impressions—if white rats have such—may have been possible to *X*; and brightness sensation until the fifth. Rat *Y* may have had brightness sensations in the first two experiments, but not later. At this time, the general health, vigor and temperament of these rats were unaffected by their malady."

Before discussing in detail the behavior of these blind rats in the maze, let us venture a word of remonstrance against the above inferences as regards the degree to which these defective rats could see. From our own observations on the vision of these animals, we feel that it is impossible to make the above sharp distinctions, as regards the very trial at which the rats became blind, and in what trials distinct impressions became indistinct. In the first place, it is next to impossible, we shall go further, it is *impossible* to tell when a white rat is blind, if he possess the organ of vision at all. Many times, we have taken our normal rats with their eyes adapted to darkness, and have flashed strong lights into their eyes—we have never got the *slightest quiver* of an *eye-lash* by so doing, much less a reaction of the rat as a whole. This is far from saying that the rats do not sense the light, but it is strong evidence against naïvely assuming that we can easily determine just the moment when our rat becomes blind, or that he is blind at all. Again, we have extirpated both eye-balls from many rats: After they recover it is difficult to tell any difference between their reactions and the reactions of the normal rats. In order to make this point more secure, we asked Professor Angell into the laboratory as a 'disinterested party.' We put three blind rats and

[1] *Ibid.*, p. 235.

two normal rats upon a large table and asked him to tell the difference in the behavior of the two groups. After an hour's observation, he stated that the difference was practically *nil;* that there was possibly a slightly stronger tendency on the part of the blind rats to rear up on their hind legs.

Small does not tell us of a single test which he made to prove his statements. But to return to the behavior of these defective rats: To be concise, they learned the maze; learned it perfectly, with as few trials and with as few errors as did the normal rats.

Small concludes from these experiments that "sight certainly is not a *sine qua non* in the process of experimental reasoning incident to these experiments."[1]

These are essentially the data by means of which Small arrives at the conclusion that the tactual-motor sensations are the important and only necessary ones used by the white rat in the recognition and discrimination of the important points in the maze.

Kinnaman, whose work on the monkeys in the Small maze has already been discussed, contributes very meager data towards the analysis of the sensory factors entering into the monkey's reactions to this form of problem. Beyond the bare, vague statements which he makes, viz., that the sense of smell is not acute (which he doesn't prove) and that the monkeys rely almost exclusively upon the sense of vision, one searches in vain for any kind of answer to the question.

Porter, in his tests upon the English sparrow in this maze, makes no express statements concerning the sensory factors used by them in the association. The inference to be drawn from his work as a whole, is that vision, in all ordinary reactions, is the sense most depended upon. Whether the sparrows or the monkeys could run this maze in the dark after being trained to it in the light, or whether they could *learn* it in the dark are points which are not touched upon in either of the papers referred to.

Some interesting efforts have been made to determine the specific sensory factors used by the different animals in the

[1] *Ibid.*, p. 236.

learning of the various simpler mazes. Yerkes,[1] in testing the green frogs in simple labyrinths with colored walls, found that the visual impressions received from the different colored walls, the slight differences in brightness of illumination due to shadows from the partitions and the contrast in form of the two sides of the labyrinth resulting from the use of the partitions, and the *muscular sensations dependent upon the direction of turning* were the important sensory data contributing to the establishment of the association. (" The experiments proved beyond question that vision and the direction of turning were the all-important factors in the establishment of the habit.") Yerkes goes on to say, that he had at first thought that the *direction of turning* was the chief determinant in the learning process, but since he found that by altering the visual conditions in the maze, the frogs' behavior was so markedly changed, he came finally to the conclusion that visual data contribute largely to the solution of the problem. Yerkes was also able to show that the tactual sensations and the organic sensations are contributory.

Yerkes and Huggins[2] make the following statements about the sensory factors used by the crawfish in his endeavors to learn the maze: " In the crawfish, the chief factors in the formation of such habits are the chemical sense (probably both smell and taste), touch, sight and the *muscular sensations resulting from the direction of turning* (italics ours). The animals are able to learn a path when the possibility of following by scent is excluded." (All the experiments cited above from Yerkes and Yerkes and Huggins were made upon normal animals. When they speak of excluding the factor of smell, they mean that they accomplished this by washing the labyrinth thoroughly after each trial.)

Triplett[3] and others who have observed fishes in captivity, have emphasized the keen visual powers of these animals. The

[1] *Harvard Psychological Studies,* I., p. 579.

[2] 'Habit Formation in the Crawfish Cambarus Affinis,' *Harvard Psychological Studies,* I., p. 565.

[3] 'The Educability of the Perch,' *Amer. Jour. of Psy.,* 1901, Vol. XII., p. 354–360.

experiments of Triplett show that perch, when separated by a glass partition from minnows living in the same tank, slowly learn to cease striking at the minnows. The many experiences which an animal gets by constantly bumping into the partition so fixes the position of it for him, that he will not cross the line of it when the plate itself is removed. We quote the following from Triplett's notes:[1] "On May 4th, glass removed in order to clean tank, but waited to see if fish would cross line. The male swam out to place, stopped, made little bumps forward as if expecting the usual obstruction and was plainly at a loss. He then turned and swam down, as if following the glass." Although Triplett doesn't say so, this behavior strongly suggests a kinæsthetic 'memory.'

Watson[2] found that young rats, 10, 11 and 12 days of age, could return to the mother over a labyrinth path. At the ages mentioned, the rats were blind and deaf. It is suggested in the experiments referred to (see note, p. 85) that the memory of such a pathway is possibly motor. The possibility that smell sensations aided in the return was not excluded. Their behavior, however, did not suggest the use of olfactory sensations.

Miss Allen[3] made some very interesting experiments upon the sensory factors used by the guinea pig in learning a simple labyrinth. She first eliminated the visual factor by forcing the animal to learn the labyrinth in the dark. Comparing the record obtained in this way with a similar one obtained in the light, we find: "(1) The range of variation in reaction time is greater in the dark than in the light; (2) a longer time is required to form a definite habit of entering the cage for food; (3) the average time required, even omitting the excessively long periods, is longer than that required for the analogous experiments in the light . . . ; (4) it follows, therefore, that the number of random movements is much greater in the dark than in the light. This the smoked paper

[1] P. 358.

[2] *Animal Education,* pp. 59 ff.

[3] 'The Associative Processes of the Guinea Pig,' *Jour. Comp. Neurology and Psychology,* Vol. XIV., No. 4, July, 1904, pp. 293–359.

showed to be almost invariably the case." Miss Allen tried to determine the effect of putting colored cards at one of the critical turns. These did not hasten the formation of the association nor, after the association was formed, did their removal disturb the reactions of the animals.

Experiments were also made for the purpose of testing the importance of contact sensations in the 'recollection' of the labyrinth path: Vision was excluded, and the contact values of the path were altered by substituting a cardboard labyrinth for the original wire one. "A black cloth was spread over the floor of the experimental cage to change still more the tactual conditions." Since under such conditions as are noted above no lengthening of the reaction time is found, Miss Allen concludes "that the path through a labyrinth is not learned solely, or even largely, in terms of tactual sensations."

The positive conclusion reached by Miss Allen from this process of elimination, is that the kinæsthetic sensations are the important sensory factors. Vision and tactual sensations are auxiliaries, but their functions can be dispensed with. Miss Allen's work suffers because of the fact that the number of her animals was so very limited.

Porter[1] makes the following statement upon the basis of his observations of the English sparrow, the vesper sparrow and the cowbird in their efforts to learn a simple maze:[2] "The behavior of the birds in this experiment tends to strengthen the opinion formed from earlier experiments with the more complex maze: namely, that, especially after the maze is learned, the birds do not depend upon sight alone for their cues as to when to turn and in what direction, but on a sense of direction and distance as well. That this is, at least in part, in terms of muscular sensations is probable."

The work of Rouse[3] remains to be cited. The experiments on labyrinth *L* are discussed by the writer somewhat in detail,

[1] 'The English Sparrow and Other Birds,' *Amer. Jour. of Psy.*, Vol. XVII., No. 2, pp. 248–271.

[2] P. 257.

[3] The Mental Life of the Domestic Pigeon,' *Harvard Psychological Studies*, II., pp. 581–613.

and since they have particular bearing upon our present question, we shall give the author's account of them.[1] In the first place, these mazes were constructed by inserting movable wire partitions in a wooden box. "On entering the labyrinth with the partitions in place the first time, a bird started on its usual course toward the food box; running against the first partition, it made vigorous efforts to push through, flying at the wire and often clinging to it for a short time; some of these random movements eventually brought it to the left of the compartment, and thence, through the opening, into the second compartment, and so on through the others, until finally it reached the food by a series of fortunate accidents. The same general reaction was shown in the case of the next few tests, except that fewer and fewer useless movements were made. . . . The great importance of visual data is brought out by the abrupt lengthening of the periods in the case of tests 23–25 and 26–30, where the light intensities were decreased. The lengthening was roughly proportional to the change of illumination. In the relative darkness, the birds had to reacquire the habits. The same mistakes were made as at first (running against partitions, and into the blind alley), yet here, as before, there was a ready adjustment. That the food was out of sight, or at least very much less visible, probably made no difference, since it was found that the birds would readily go to the old place after both food and food-box had been removed. In order to exclude the light entirely without making their movements invisible to me, I blindfolded the birds by means of a thin black hood, comfortably (?) adjusted over their eyes and top of head; as a result, none was able to make the course in twenty minutes. The first turn, however, was usually made naturally, perhaps because associated with certain non-visual sense-data (sound of the lifting door, and perhaps tactual impressions of the close entrance compartment, etc.)." And just here, at the point where we are becoming interested in his general analysis, he stops it. But let us look a little closely at the data which he uses in drawing the conclusion that visual sensations are of such great importance in the labyrinth.

[1] P. 587.

Why did the birds in the beginning of the learning process flop against the partitions if vision is so all-important? Would they have done so, if the partitions had been of boards and therefore made opaque and more easily visible? Again, if one refers to Rouse's table on page 589 of the article referred to, where the time of the learning process is shown together with the changes in the normal time, produced by the decreasing of the intensity of the light, one finds: 1st, That the effect of the substitution of a 2 C. P. light for the 18 C. P. (the illmination used in the normal learning process) is felt for *only one trial after the substitution is made;* 2d, that the almost entire exclusion of the light ('with a slight illumination through a single curtain, other conditions the same'—is the way Rouse states it) produces marked disturbance upon the pigeons' reactions for only three trials, the fourth trial after the change being practically normal, the fifth trial being quite so. If the pigeon is using *visual discrimination* in the maze to any extent, is it likely that the readjustment made necessary by the great change in the illumination could have been effected in *one trial* in the first case, and in *three to five trials* in the second case? And again, Rouse makes no attempt to separate the tonic and emotional effect of the light from the purely cognitive. May we not suppose that the poor records in these first three trials were due, in part at least, to the presence of fear or excitement and that under the influence of this emotion they reverted for a moment to the instinctive type of behavior? What would have happened if Rouse had forced his birds to live in the dark for a few days, regularly taking their meals there—would they not have learned the labyrinth in the dark? Finally, again referring to his experiments quoted above—did the putting on of the hood show anything? In the first place Rouse fails to state a very essential point in connection with this experiment: for how long were the birds forced to wear this comfortably (?) adjusted hood before they were introduced to the labyrinth? Since no statement is made concerning this, the presumption is that he hooded the pigeons and tested them immediately. If he did this, nobody would expect any one

of them to run the maze in 'twenty minutes.' Finally, 'twenty minutes'—even if the condition of the birds had been made comfortable, is not an exceedingly long time to wait for a blindfolded animal to readjust himself to a situation he knows only 'visually.'

We have cited these results at some length because they are, in so far as the time factor is concerned, so similar to the ones we obtained by removing the vibrissæ of the rats trained to the maze (see page 69). Our conclusion there, however, is that while the sensations from the vibrissæ are doubtless used by the rat in his *immediate adjustments* to the various turns in the maze, still they can easily be dispensed with—and certainly are not used as the basis of the discrimination of the turns nor are they at all necessary to the learning process as a whole. Indeed, it is a little hard for us to see why this same statement of the case does not apply to the pigeon in regard to his use of vision in the maze—at least, Rouse has produced no clear proof to the contrary. He has not proven, in our opinion, that the pigeon makes his turns on the basis of visual discrimination.

In certain other experiments, Rouse proves conclusively that these birds possess acute auditory and tactual organs, and that impressions from these avenues play a large part in the associations of the animal. The apparatus used in reaching these conclusions consisted of two especially constructed labyrinths: one arranged to give the pigeon qualitatively different auditory stimulations at certain turns; the other, to permit similarly of electro-tactual stimulation.

In conclusion, we may say, that while Rouse does not deny that sensations from turning, etc., are utilized by the pigeon, yet he everywhere implies that the adjusting movement is mainly released by the visual impulse. His final conclusion is (page 612): "Visual, acoustical, probably tactual, and certainly organic data are the principal sensory factors of the associations of the pigeons."

Miss Fielde's study of the behavior of the ant[1] *Stenamma fulvum piceum,* in a small but fairly complicated maze, shows

[1] *Proceedings of the Academy of Natural Sciences of Philadelphia,* 1901, pp. 521–544.

that these ants are guided mainly, in their successive trips through the maze by the smell of their own individual trails. Consequently, according to her report there is almost no ‘ short circuiting ’ tendency present in this species of ant. ‘ Errors ’ are not eliminated. On being admitted at the pole of the maze opposite to its nest, the ant makes excursive journeys until the nest is found. If, at repeated trials, the ant is put down at the starting-point, the old trail to the nest is followed with increasing rapidity. Miss Fielde thinks that ‘ memory ’ also plays a part in the return. She found that after the animal had traversed its own trail for a number of times, she could remove increasingly larger portions of the trail without causing any disturbance in the animal's reactions. " This proves," says Miss Fielde, " that the ant does not smell her way at every point, and that familiarity with certain objects under her feet is gradually acquired."

In view of the fact that the literature concerned with the general behavior of the ants and their sensory equipment is so vast and so controversial, and that the order of the ant is so far removed from that of the higher birds and the mammals, we think it wisest not to attempt a further review of the research papers concerning them. We should like to say in passing, however, that no field in comparative psychology is more interesting nor more fruitful than that of the behavior of the various species of ants, and yet there is no field lying more in confusion. Nowhere in the literature of this subject does one get a central, psychological point of view. It is an interesting fact but true, that every man from the layman to the metaphysician on a vacation, has studied the " instincts and habits " of ants—every one, except a well-trained comparative psychologist! This statement, of course, has no reference to purely biological studies.

(*b*) *Dr. Carr's Experiments with the Maze in Darkness.*

1. *Upon Rats which had Learned the Maze in the Light.* —Dr. Carr trained five rats—three young and two adults— to run the maze perfectly *in the light.* When the rats had

reached a fairly constant and minimum time for running the maze in the light (this takes about 30 trials by our method, viz., giving the rats 4–6 trials each day), it was decided to test their reactions to the maze in darkness. The record of the last five trials of these rats in the light was used as a basis of comparison with the five trials taken in the evening of the same day.

The method of conducting the experiments with the maze in darkness was as follows: First, they were made at night; second, the heavy shades were pulled down over all the windows; third, a single 16 C. P. electric light was left burning in the room until the rat was put into the entrance enclosure to the maze. The instant the rat took his cue and dashed down the gallery, the light was noiselessly and quickly turned out. A little platform attached to an electric buzzer at the entrance to the food-box signalled the end of the rat's journey. A continuous stop-watch, muffled by placing it in the pocket, was used in recording the time. For several days during the training process in the light, the rats had been forced to cross the little platform at the entrance to the food-box. The rats soon became accustomed to the drop of the platform and to the ringing of the electric bell. The procedure adopted with reference to the use of the electric light was deemed important in that it would tend to eliminate adaptation to the dark. Of course, we can't say that adaptation to darkness was complete in the case of the rat; but we can say that during his passage through the maze the room was absolutely *pitch dark* to the human observers present. Unless the process of adaptation goes on enormously more rapidly in the retinæ of the rat than in the human retinæ, the maze must have been absolutely dark to the rat. Below, we give the comparison records—the one, the five trials in the light—the other, the five trials given four hours later in the dark.

Judging from these results alone, it would apparently take a microscope to find the influence of vision in the maze association! Four out of the five rats made the trips in the dark in an absolutely shorter time, while the remaining rat (III.)

made the trips in a time approximately equal to the comparison trials in the light.

MAZE IN LIGHT.

Rats.	I.	II.	III.	IV.	V.
Trials.	Min.	Min.	Min.	Min.	Min.
1	.40	.36	.29	.30	.36
2	.41	.25	.50	.24	.24
3	.46	.33	.25	.24	.24
4	.53	.60	.24	.30	.28
5	.70	.40	.25	.66	.70

MAZE IN DARK.

1	.50	.40	.31	.31	.25
2	.33	.25	.50	.26	.30
3	.41	.25	.25	.24	.33
4	.56	.35	.24	.25	.24
5	.41	.45	.24	.29	.30

AVERAGES FOR THE FIVE TRIALS IN THE LIGHT AND THE FIVE IN DARKNESS—EACH RAT SEPARATELY.

In light.	.500	.388	.306	.348	.364
In dark.	.442	.340	.308	.270	.302

2. *Upon Rats which were Forced to Learn the Maze in Darkness.*—There are two obvious criticisms to be made upon the results of the above experiments. In the first place, adaptation to darkness might have occurred; second, it may be objected that the rats had been so thoroughly trained to the maze in the light that they could do it 'hands down' and with 'their eyes shut'! *i. e.*, automatically. In other words, it may be said truly that the experiment does not prove that the rat *does not use* vision even largely in the early stages of the learning process. The first objection can be met only by extirpation of the retinæ. The second possible objection was partially met by Dr. Carr in the following way: The animals were forced to *learn the maze in the dark* from the very beginning. If vision is necessary or important or used to any appreciable extent, the fact ought to show itself in the *increased time of the learning process.* The following table and the accompanying graph (Table IV., Curve IV.) show the record made by Dr. Carr's rats in learning the maze in the dark. It is based upon three young rats and is to be com-

pared directly with Table II., and Curve II., the corresponding records for three young rats in the maze in the light.

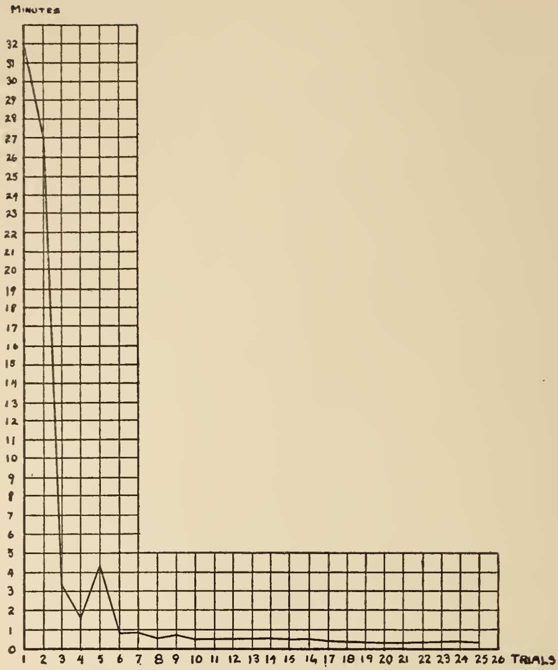
(TABLE II.[1] Showing Dr. Carr's experiments on the learning of the maze by three very young rats.)

TABLE IV. Showing Dr. Carr's experiments upon three rats forced to learn the maze in the dark.

No. of Trial	Average. Minutes.	No. of Trial.	Average. Minutes.
1	9.76	1	31.94
2	22.50	2	27.05
3	3.70	3	3.45
4	3.46	4	1.57
5	2.08	5	5.34
6	1.58	6	.83
7	1.30	7	.88
8	.99	8	.63
9	1.15	9	.75
10	1.05	10	.49
11	1.02	11	.50
12	1.55	12	.52
13	.41	13	.63
14	.51	14	.44
15	.44	15	.40
16	.53	16	.44
17	.33	17	.40
18	.45	18	.37
19	.48	19	.34
20	.48	20	.30
21	.49	21	.29
22	.26	22	.30
23	.33	23	.30
24	.30	24	.34
25	.33	25	.27
26	.35		
27	.50		
28	.30		

The main differences to be found in the two sets of records are as follows: The first two records in the dark are higher than the corresponding ones in the light—and why? Simply because we made a slip in our technique. The rats were fed in the food-box for a few days in the usual preliminay way, but in the light; unfortunately, they were not fed *at the time* they were later to be experimented upon, viz., at 8 P. M. As a consequence, when Dr. Carr took the first two records (both the first evening) the rats were not eager and actually curled up and went to sleep in the maze! This time, again unfortunately, was not taken out. The second noteworthy point about the record in the dark is the fact that it is very much superior, if we neglect the first and second trials, to its mate in the light.

[1] For convenience of comparison Table II. is here repeated. See p. 21.



Curve IV. Showing the record of three rats forced to learn the maze in the dark.

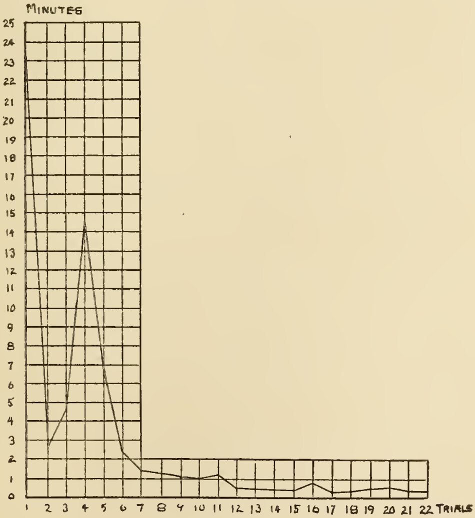
(*c*) *Dr. Carr's Experiments Designed to Obtain Positive Evidence of the Rôle of the Kinæsthetic Sensations.*

Since the above experiments all tended to support Small's contention, viz., that the tactual-kinæsthetic sensations play the leading rôle in the formation of the maze association, we determined to see what the effect would be if the conditions obtaining in the maze were changed in such a manner that the possible tactual-kinæsthetic impressions would be made more intense. Accordingly, we cut small blocks of wood which would just fit across the galleries; holes, one inch from the bottom edge, were bored in the blocks barely large enough to admit the body of the rat. These blocks were placed in the entrance to the true pathway at every point where the maze offers a choice of turns. *A priori,* it might be thought that the squeezing movements necessary to force the animal through the blocks would bring the kinæsthetic factors into such relief

that the time of the learning process might be considerably decreased. The appended table and its graphical representation show that if anything the learning process is made slightly more difficult. It is certainly ambiguous. A variety of causes may lie at the bottom of this condition of affairs. In the first place, the animal is actually delayed, much like a hurdler, by having to squeeze through these blocks; in the second place, the squeezing process may be highly unpleasantly toned; and finally, owing to our failure thoroughly to accustom the animal to getting through these blocks before beginning the experiment, the emotion of fear was aroused.

TABLE V. Showing the learning of the maze when blocks are inserted at all correct turns where a choice of turns is offered.

No. of Trials.	Average. Minutes.	No. of Trial.	Average. Minutes.
1	23.25	12	.58
2	2.64	13	.51
3	4.60	14	.45
4	14.62	15	.47
5	6.62	16	.83
6	2.37	17	.46
7	1.50	18	.48
8	1.37	19	.59
9	1.16	20	.61
10	1.09	21	.42
11	1.37	22	.48



CURVE V. Constructed from Table V.

(*d*) *Partial Repetition of Dr. Carr's Experiments by the Present Investigator.*

After we had trained the four rats whose records (normal control series) are shown on page 19, we decided to repeat Dr. Carr's experiments with the maze in darkness. Below, we append side by side the control record in the light, and the comparison record made 4 hours later in the dark.

	In Light.		In Darkness.	
	Trial.	Min.	Trial.	Min.
Rat I.	1	.28	1	.30
	2	.38	2	.31
	3	.36	3	.48
	4	.38	4	.27
Rat II.	1	.25	Total failure. He was tried five times—and was allowed five minutes at each trial. He was hopelessly lost in the dark. Each time the light was turned up he was found wandering aimlessly about in the cul-de-sacs. The moment the light was turned up he would find the true pathway and dash to the food. On the following night he was again tried three times, five minutes for each trial, but failed utterly each time. He never failed in the light.	
	2	.25		
	3	.25		
	4	.56		
Rat III.	1	.25	1	.25
	2	.23	2	.20
	3	.25	3	.18
	4	.21	4	.20
Rat IV.	1	.30	1	.20
	2	.23	2	.17
	3	.23	3	.20
	4	.26	4	.17

With the one exception, these results agree with those of Dr. Carr.

Rat II. being an anomaly in these experiments with the maze in darkness, it was decided to test him further.

Professor Angell suggested that the contrast in brightness between the walls of the gallery and the opening made in it at the turns might serve possibly as an 'eye muscle pull,' *i. e.*, that the orientation might still be mainly through kinæsthetic impulses, but, in this rat, they might arise largely by the eye-muscle strain rather than by the general turning movements

of the body and limbs. This suggestion, of course, would be far from denying that the visual impulse plays a part. If it were true, however, it would do away with the necessity of assuming that the rat makes his turns on the basis of visual *discrimination*.

The apparatus for testing this theory was arranged as follows: At all turns in the maze, small miniature lamps were installed (voltage 3½, amperage .19, C. P. in full illumination approximately 3) at the eight critical turns, in such a way that the light would shine from behind directly out through the proper opening into which the animal should turn. These lights (arranged in multiple arc) were connected with a portable sliding rheostat, which in turn was connected with the ordinary 112 volts direct lighting circuit. By means of the rheostat, the little lamps could be changed in intensity from 0–3 C. P. If the above assumption were correct, the faintest glow in the wires of the miniature bulbs ought to be sufficient to produce the necessary eye-muscle pull. After the apparatus had been arranged, the rat was run through the maze for several days until the miniature lamps and their insulated wires ceased to be a matter of interest to him. We shall quote the results of these experiments from our diary.

2/24/06. (1) Rat II. was again tried in the dark for three minutes. He made very many errors by running into practically all the cul-de-sacs. (Permit us to say, that after one has worked with a single maze for two of three years, one can close one's eyes, if the room is quiet, and by the sound that the rat makes in his passage can tell the exact position of the rat in the maze. The writer trained himself to do this, having this very end in view.) At the end of this time, having made no material progress, he was removed from the maze.

(2) The miniature lights were then turned on so as to show only a faint glow. The rat was next admitted. He was materially aided from the start. He would pass the true entrance for a little way, stop and then *invariably* turn into it. Unfortunately, we lost the absolute time of this trial. It was not longer than .75 min.

(3) Time: .50 min. Hesitancy as before but no errors.

(4) Time: .30 min. Clearly and definitely done.

(5) The rat was then tried in absolute darkness. After 2.18 min. of fruitless wandering, he was taken out.

(6) With miniature lights again. Time: .30 min.

2/25/06. (1) The rat was first tried in the dark for 1.55 min. He made a total failure of it.
(2) With miniature lights. Time: .20 min.
(3) With miniature lights. Time: .21 min.
(4) With miniature lights. Time: .23 min.
(5) In the dark again. No success at the end of 2.06 min.
(6) With miniature lights. Time: .30 min.

2/26/06. We were definitely interested by this time to see if the beast could even *learn* to manipulate himself in the dark. We tried him for 33 min. He was eager and worked hard but at the end of that time he gave it up. The room lights were then turned up. He took his cue like a flash. Time: .33 min.

2/27/06. Rat very hungry. Repeated experiment in the dark. No success at end of 24 min. He was then tried with room light turned on.
(1) Time: .33 min.
(2) Time: .33 min.
He was tried in dark again. He was eager for a time but gave up after 12 min.

2/28/06. We began tonight determined to make the rat get to the food in the dark if it took all night.
(1) Success! Time: 10.22 min.
(2) In dark. Time: .65 min.
(3) In dark. Time: 6.85 min. Tried to gnaw into food-box.

3/1/06. Five trials given with the maze in darkness.
(1) Success. Time: .26 min.
(2) Success. Time: .23 min.
(3) Success. Time: .38 min.
(4) Success. Time: .20 min.
(5) Success. Time: .28 min.

3/2/06. Conditions the same.
(1) He got hopelessly lost in the dark. Returned to starting point again and again. Both active and hungry. Time: 12 min.
(2) Ceaseless wanderings as in first trial. Time: 5.35 min.
(3) Time: .66 min. Some few errors were made.
(4) Time: .96 min. Some few errors were made.

3/3/06. Conditions the same. Four trials in the dark.
(1) Time: .20 min. Splendid work.
(2) Time: .23 min. Splendid work.
(3) Time: .25 min. Splendid work.
(4) Time: .23 min. Splendid work.

3/11/06. Unavoidably, several days elapsed without trial. In dark.
(1) Time: .31 min.
(2) Time: .28 min.
(3) Time: .41 min.
(4) Time: 10.00 min. Hopelessly lost again.

3/12/06. Conditions the same. Four trials in the dark.
(1) Time: .25 min. Very consistent work.
(2) Time: .20 min. Very consistent work.

(3) Time: .41 min. Very consistent work.
(4) Time: .43 min. Very consistent work.

3/13/06. Conditions the same. Four trials in the dark.
(1) Time: .20 min.
(2) Time:: .20 min.
(3) Time: 11:00 min. Lost.
(4) Time: 7.48 min. Lost.
(5) *In light.* Time: .26 min.

3/15/06. Conditions the same. Four trials in the dark.
(1) Time: .20 min.
(2) Time: .50 min.
(3) Time: .50 min.
(4) Time: 1.03 min.

We despaired at this point of ever getting the rat to reduce his time to a low and constant one. He has definitely improved but he is not consistent even after a number of trials.

The following summarized statements may be made concerning the behavior of this rat: (1) His behavior in the maze in the dark is in marked contrast to that of the eight other rats tested under identical conditions. (2) The assumption that the sensations arising from the changes in the brightness intensity at the critical turns *associated with certain strain sensations* arising in the eye-muscles serve as the releaser of the motor impulse, is the simplest one which will fit the facts—it being far more logical, in view of the above experiments, than the assumption that orientation is made on the basis of visual discrimination alone. (3) There is a final possibility; this rat was not quite so strong and hardy as the other rats. The effect of the light might have been general and stimulatory rather than specifically visual, *i. e.,* visual in the ordinary cognitive sense of the term.

From the above historical survey of the field, and from the report of these, hitherto, unpublished experiments made in this laboratory, it becomes evident, that if our analysis of the sensory factors necessary or contributory to the formation of the maze association is to be complete or convincing, we must make the analysis from data obtained from the behavior of rats whose visual, olfactory and auditory organs have been extirpated, for only in this way can we become even reasonably certain that impulses from a given sensory pathway have

been excluded. Neither the experiments of Small, nor those made in this laboratory prove conclusively that vision plays no rôle in this association—it stands as a criticism against our own work (and Dr. Carr's) that we have not excluded the possibility of adaptation to darkness; the sense of smell may play the preponderating rôle after all—even Small suggests that the behavior of rats whose olfactory nerves have been paralyzed ought to be observed in order to make his own inferences, obtained from the observation of their normal behavior in the maze, more secure; while it is probably not true, it is at least not utterly fantastic to suppose that auditory sensations (or, more strictly, tactual sensations aroused by the effect of the changing pressure of the columns of air upon the tympanic membrane) may contribute some data; pure tactual sensations may *at least* be contributory—especially those arising in the soles of the feet and in that delicate and ever-moving set of vibrissæ; the rat may have some distance sense—'facial vision,' 'temperature' or what not—which the human organism possesses only in a comparatively rudimentary form.[1] Finally, while ridiculous from the standpoint of human behavior, it is not wholly ridiculous in the case of the rat, to suppose that gustatory sensations contribute data—at least this might be the case where the animal is denied certain other afferent impulses, and thereby is forced to rely more upon the few sensory avenues which an unkind providence has left him.

Believing that the observation of rats with defective sense organs would aid us in returning a more or less complete answer to these and similar questions, we decided to extirpate the sense organs of vision, olfaction and audition.

In the spring of 1905, while the writer was in Baltimore, he began experimenting upon methods of operation designed to remove the special senses in these animals, or where removal was impracticable, to decrease very greatly the sensitivity of the function of these organs. The work was done under the direction of Dr. Howell, Director of the Physiological Laboratory of Johns Hopkins University. The writer wishes here

[1] From certain experiments upon the rotated maze the possibility arises that static (?) sensations may likewise be contributory.

to express his obligations to Dr. Howell and to the members of his staff, both for their personal direction and for the use of their laboratory. The animals operated upon at Johns Hopkins were not used in the experiments. On our return to the University of Chicago, in October, 1905, the operations immediately to be described were made upon tamer animals than could be obtained in Baltimore.

II. *Operations to Remove the Senses of Vision, Olfaction and Audition.*

(*a*) *Method of Operating when the Eyeball is Removed; the Recovery from this Operation.*

In all of the operations which we are now about to describe, aseptic precautions were taken when possible. It is impracticable, however, in operating upon an animal as small as the rat to use all the refinements adopted in modern surgery. Fortunately with the rat this makes very little difference: He seems to be able to withstand almost any amount of exposure to sepsis and show no ill effects. In none of these experiments was any sign of infection ever noticed. The white rat takes the ether extremely well. In the beginning, before the technique of the more difficult operations was well established, our rats were often under the influence of ether from 1 to 1½ hours.

In view of the fact that the removal of the whole eyeball does away with any possibility of even vague brightness sensations, we chose this method rather than that of enucleating the lens. It was feared before the operation that grave trophic disturbances of the metabolism of the animal as a whole might result from the total removal of the influence of the optic tract, but we thought best to try the method at any rate. *Our animals invariably recovered, nor did they show any signs either of immediate shock effects or of slower and more remote systemic effects.* Three animals operated upon in this way more than five months ago are still in good condition.

In removing the eyeball, the animal was first made completely anæsthetic. The vibrissæ and longer hairs were then

removed. The whole eye-ball, lids, etc., were next thoroughly cleansed. A small, especially constructed hook was inserted into the sclerotic and by means of this the whole eyeball was pulled forward. With fine, sterile scissors, the connective tissue and muscles were cut away. This can be done practically without hemorrhage. The eyeball was then pulled still further forward, thus exposing more completely its attachment to the optic tract. This was next cut away with one stroke of the scissors. A rather profuse hemorrhage, produced by the sectioning of the ophthalmic artery, then occurred which we did not attempt to check. In a short time, this ceased of its own accord; the cavity was next thoroughly cleansed with sterile normal salt solution. The second eyeball was then removed, additional ether being administered by means of a cone if necessary. The animal was then put into a small freshly cleaned cage and allowed to recover. No effort was made to close the site of the wound or to restrain the animal from scratching it. Of course, in this way, we ran the risk of having infection develop. It is almost impossible, however, wholly to prevent the possibility of this in the eye operation.

In order to show the almost total absence of any shock effects resulting from the above described operation, we shall quote the following from our notes:

"Four male rats, all from one litter, born September 10, 1905, were operated upon March 24, 1906. Both eyeballs removed. The immediate shock effects were almost *nil*. The rats accepted food six hours after the operation. Twenty-four hours later they had thoroughly cleansed themselves. Motor control perfect. Several difficult feats were performed during this period of observation, such as balancing upon the rim of their little 8″ by 4″ wire cages. They walked around the edge of these cages with ease. They balanced themselves upon a small stender dish containing water—hind legs on dish, fore legs in air. One rat obtained some food, a second followed, bowled the first rat over, felt in his mouth and removed the food contained there. Two other rats were seen to play!

"48 hours after operation: The rats were absolutely normal so far as we could judge. Motor control perfect. Appetite

fine. Eager curiosity exhibited when the rats were released. No signs of irritableness. These rats, even at this early stage, do not run with heads any higher or any lower than normal rats. If we could not see that their eyes had been removed, it would be absolutely impossible for us to tell them from the normal rats!

"72 hours after operation: Wound shows no sign of infection. Appetite splendid. They scramble and fight for food.

"96 hours after operation: Hale and hearty. All four rats engaged for a long time in a playful rough and tumble fight. Movements characteristic of the combats indulged in by normal rats were made perfectly; one rat falls down while a second stands over him and grasps his throat or mouth.

"5 days after operation: All four rats were transferred to one large cage. The food and water in it were found without useless movements. The storing instinct was exhibited. One rat was observed to sit near a pile of sunflower seed: at intervals, he would stretch out his neck and help himself. He never made a mal-coördination by over or under innervation nor by moving his head too far, either to the right or to the left."

Enough has been quoted from the diary, we think, to show that the shock effects from this operation are so mild as to be hardly noticeable.

(*b*) *Method of Operating when the Olfactory Bulbs are Removed; the Recovery from this Operation.*

Since it is impracticable to remove the peripheral olfactory nerves, we decided that the easiest way to exclude the possibility of reactions to olfactory stimuli was to trephine and actually to remove the bulbi olfactorii. This is an exceedingly easy operation due to the fact that the bulbi lie entirely cephalad to the frontal lobes and not ventral to them, as can easily be seen from the appended cut.

The bulbi are well developed and their line of junction with the frontal lobes can easily be seen through the cranial bones, after the skin incision has been made. This junction point

lies on the line connecting the posterior angles of the lids of the two eyes. With a very small, well-tempered trephine drill a series of tiny holes was made in the form of an oval around

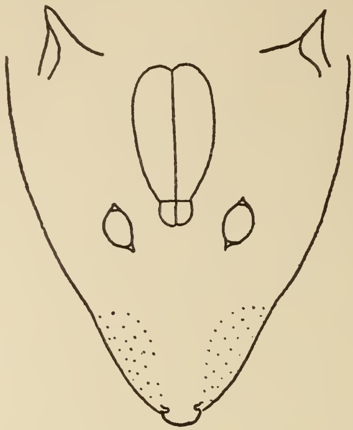


FIG. 2. Showing the relation of the bulbi olfactorii to the hemispheres and to the head as a whole. (Life size.)

the two bulbi. The little island of bone was then removed. If care is taken not to injure the frontal sinus in the drilling process, the bulbi can be exposed entirely without the loss of a drop of blood. A small, especially constructed, double-edged knife was next inserted at the junction of the bulbi with the frontal lobes. This knife was forced vertically down to the supporting floor of the bulbi and then driven from left to right until a complete separation between the bulbi and the frontal lobes was effected. With a very small eye curette, the bulbi thus severed were rapidly and thoroughly removed. Naturally, a profuse hemorrhage occurred, due principally to the sectioning of the frontal sinus. The actual loss of blood during the hemorrhage varied greatly from individual to individual. This accounted in part for certain differences in the behavior of the rats during the period of recovery. By gently tamponing the cut ends of the vessel, the hemorrhage was soon controlled. The cavity thus made was thoroughly washed with sterile salt solution and then sponged. Any material of the bulbi remaining from the first curetting process was removed. The operation *in all cases* was made absolutely com-

plete. After the checking of the hemorrhage, and the sponging process following upon this, the whole cavity was blood-free and remained so, thus affording a complete view of the operated field. After the removal of the bulbi had been completed, the skin flaps were stitched together; collodion being applied along the line of incision. The animals were then put into individual, sterile cages, and kept there until recovery was complete.

In this case recovery is slower than in the visual operation. The animals are more irritable and pugnacious than usual and show a disposition to strike their heads against the sides of their cages. The contraction of the collodion which covered the site of the wound may account for this behavior. They seem unusually sensitive to light from which they attempted to protect themselves by stuffing cotton into the meshes of the cage on the side from which the light came. Four weeks after the operation the wound had so completely healed over that had we not marked their ears we should have been absolutely unable to identify them among the normal animals. Needless to say, their behavior gave us no clue whatever.

(*c*) *The Method of Operating when the Middle Ear is Removed; the Recovery from this Operation.*

Thoroughly to eliminate the influence of air vibrations, one must cut the cochlear branch of the eighth nerve or else destroy the inner ear. Both of these operations are exceedingly difficult to perform with sureness and certainty.

For our purposes, neither was supposed to be necessary. In the first place, it was thought that if the slight noises made by the animal in his passage through the maze aided him in making his turns at the critical places, then the mechanism by means of which this is done must be exceedingly delicate, for, as has been stated, the sounds made by the rats in the maze are very faint; and the differences in the reflection of these sounds (which, if this were the *modus operandi,* would be the only way in which a discrimination could be made) would be fainter still. The conditions under which the normal animals had to learn the maze ought, however, to rule out of court the supposition that air waves (*i. e.,* those producing the con-

ditions for the arousal of an auditory sensation as such) influence the reactions of the rat to the maze. Briefly, the conditions are these: The one large room where these experiments were all made, is surrounded on the one side immediately by a large ventilating fan and its motor, on the other, by a machine shop—a peculiarly active place, containing a motor, an emory wheel and an instrument for grinding, boring and polishing glass. In addition to these rather constant noises, there were others coming from a large number of rats kept in the same room with the maze. To any one familiar with 'rat noises,' no further description of the immediate surroundings of the maze need be given.[1] After this description of the noises in which the rats had to learn the maze, we venture to say that no one would have the hardihood to suggest that the sensations of sound produced by the rat himself could influence his behavior in the maze.

The aid from the aural organ, if any aid is contributed by it, would presumably more probably arise from the effect of the changing pressure of the columns of air upon the tympanic membrane. Doubtless these columns of air do change as one passes from a long gallery into a short one, as one approaches an opening into a gallery, or finally, as one approaches the end of a cul-de-sac. The important question is, has the rat any sensory mechanism in the ear drum, or in the skin of the nose, wherewith to sense these changes in the density of the air?

By the destruction of the drum membrane, the removal of the chain of bones and the filling of the middle ear cavity with paraffine, the aural mechanism which would be likely to become functional under the influence of the changes in air pressure is placed *hors de combat.* In addition to this advantage, the above operation for a time, at least, leaves the rat almost absolutely deaf.

This operation can be made quickly without the loss of one drop of blood. The animal is anæsthetized, placed on one side, and a small skin incision is made running vertically down from the ventral limit of the concha of the ear. This exposes

[1] *Cf.* Porter's difficulties at Clark University! *Am. Jr. Psy.,* Vol. XVII., No. 2, p. 248.

more thoroughly the cartilaginous tissue of the meatus. An incision is made horizontally through this tissue of the meatus. Small artery clamps are attached to the concha and to the meatus in such a way that the tissues of both are pulled away, exposing the drum membrane and the handle of the hammer bone. Two tiny forceps are next employed, one to serve as a speculum, the other to destroy the drum membrane and to break up the system of bones. No effort was made actually to remove the bones; sometimes one can do this, but more often one loses them in the middle ear cavity. An especially constructed long needle-pointed medicine dropper was then used to inject the paraffine. The paraffine had been previously filtered and sterilized. By melting hard and soft paraffine together, a final melting point of 40° C. was obtained. The entire cavity of the middle ear is then filled with paraffine. The skin and cartilaginous incisions are next sewed up and the site of the wound covered with collodion.

The recovery of the animals from the above operation is rapid. We cite the following statements from our notes:

" 8/9/06. Middle ear of two females removed. Recovery from anæsthetic rapid.

" 1 hour after operation. No motor disturbances of any kind apparent. No reactions to sound could be obtained; the prolonged and shrill noise mentioned above, which gave 'the shivers' to the normal rats, produced not the *slightest sign of movement in these rats.* Noises deep and high produced by striking the various objects in the room were without effect.

" 24 hours after operation. The shock effects have practically disappeared. The animals show hunger and the usual amount of food is consumed. Extensive tests were made today with the Edelmann whistle. Normal rats are very sensitive to the sounds produced by this instrument. We have been able to obtain reactions with it throughout all the ranges of its pitch up to 17,000 vibrations per second. With these defective rats, we could obtain no sign of response to any of its tones.

" 6 days after operation. The rats are in fine condition. They were fed today in the food-box of the maze for the first

time since the operation (they had been fed there for seven days previous to the operation). Some slight signs of the return of the auditory function were observable today. The tests were made with an ordinary call-bell (this bell gives a loud, clear, distinct note whose pitch is 1200 S. V.). The normal rats react to this bell by a violent, backward movement of the whole body. An instant of inhibition follows which keeps the animal 'stock still.' The defective rats, when they noticed it at all, reacted to it with a barely perceptible shake of the head—their other movements (examination of surroundings, etc.) were in no case disturbed by the stimulus."

In all these tests upon the extent to which these rats are sensitive to sound, care was taken to have more than one observer present. All agreed that the rats' sensitivity to sound was enormously decreased by the operation. We ourselves were satisfied that the operation was perfectly complete for all the purposes concerned in the present investigation.

III. *The Behavior of Defective Rats in the Maze.*

(*a*) *The Behavior of Rats Trained to the Maze with Sense Organs Intact, After the Loss of Vision.*

Our first test upon the behavior of blind rats in the maze was made upon three animals which had *learned the maze with sense organs intact*. The original record of the learning of the maze by these rats has already been shown in the 'normal control series.' Four rats were used to obtain this series, but it will be remembered that rat II. was an anomaly as regards his tests in the dark. We decided not to operate upon him, but to keep him as a control animal, if such should be needed. Before describing these further experiments upon the blinded rats, let us recapitulate their experiences in the maze. In their normal condition, they had been given 50 trials in the light. We then tested them to see how well they could run the maze in the dark. It was found that rats I., III. and IV. could run the maze as well in the dark as in the light. In our discussion of these results, we decided that adaptation to darkness might possibly have aided the rats in their journey through the maze.

The only way absolutely to exclude this factor was to remove the retinae of these animals. Accordingly, they were operated upon in the above described manner. Their recovery was rapid and complete. The rats were not again tested in the maze until 25 days after the operation.[1] The 37 trials given the blind animals are shown in Table VI.; Curve VI shows the first ten of these trials in graphic form. This table and curve are to be compared with Table I. and Curve I. See p. 19.

TABLE VI. Showing the effect of the loss of vision upon three male rats trained to the maze with sense organs intact. The *average, minimum* and *maximum* times are given.

No. of Trial.	Average. Minutes.	Minimum. Minutes.	Maximum. Minutes.
1	2.39	1.83	3.00
2	.95	.76	1.13
3	.85	.66	1.01
4	.65	.41	1.06
5	.66	.50	.96
6	.51	.35	.71
7	.42	.20	.58
8	.34	.25	.41
9	.33	.30	.38
10	.30	.26	.36
11	.52	.33	.71
12	.52	.33	.73
13	.39	.25	.46
14	.29	.25	.33
15	.38	.30	.45
16	.38	.25	.58
17	.31	.20	.45
18	.46	.26	.71
19	.28	.28	.30
20	.27	.23	.31
21	.25	.20	.30
22	.28	.20	.38
23	.36	.20	.50
24	.25	.20	.31
25	.31	.25	.41
26	.38	.23	.53
27	.23	.20	.26
28	.25	.21	.30
29	.25	.23	.28
30	.27	.23	.30
31	.27	.25	.30
32	.23	.21	.26
33	.27	.23	.33
34	.24	.21	.26
35	.24	.20	.28
36	.25	.25	.25
37	.27	.26	.30

[1] In all cases ample time was given for complete recovery before the animals were tested in the maze.

An examination of these records will show that the average of the first trials of the three rats is the only one requiring over one minute. If one compares these records of the blind rats in the maze with their original records in *learning* the maze,

CURVE VI. Based upon Table VI.

one cannot avoid the conviction that the rats' ability to run the maze after he has once learned it is little affected by the loss of vision, if it is affected at all thereby. The record of the blind rats would have been better undoubtedly if we had properly controlled the conditions for their first few trials in the maze subsequent to the operation. In the first place, the rats were given all they could eat during the 25 days allowed for convalescence. Consequently they were exceedingly fat when we tried them in the maze for the first time. We should have starved them, or rather, have fed them lightly in the food-box of the maze for at least *one week* before allowing them to traverse the maze. In the second place, the blind rats were badly frightened by the noise and movement of the trap (used in experiments with the maze in darkness) which we had allowed to remain in the maze, for fear its removal would change the olfactory conditions. The rats would come up to the plane and touch it, but the moment they felt it move they turned back—finally mustering courage, they ran over it, but the jolt they received from it coupled with the noise made them extremely cautious when again nearing it.

We finally decided to remove the trap. In its place, we inserted a new, smooth floor in the gallery. We accustomed the rats to this new condition of things by shutting them up from the rest of the maze and forcing them to run over the new floor for several trips. An interesting bit of behavior came to light. The rats went on jumping over the *now perfectly even floor* just as they would have done if the trap had been present. They repeated this many times even after we

had started them regularly to traversing the maze. In order that the above statements concerning the behavior of these rats may be illustrated, we quote the following individual records from our diary:

"Trained rats in the maze after destruction of vision.

"Rat I., 1st trial. (Rat I. in originally learning the maze was very timid. This was evidenced by his creeping round corners and hugging the sides of the galleries (see maximum time for 1st trial in the normal control series, page 19). In this first test after the loss of vision, he reverts to his original type of behavior.) He evidently was not very hungry. *He ran the first half of the maze absolutely without error. He then turned and retraced his way to the entrance, again without error. He turned at the entrance, got his cue and went to the food-box without error.* Total time: 2.35 min.

"Rat IV., 1st trial. Walked very slowly. Made several errors in *A*, *B* and *D*, apparently out of 'curiosity.' 'Back tracks.' Time: 3.00 min.

"2d trial. Walked slowly but *did not make an error.* Time: .76 min.

"3d trial. Hesitancy (but no error) at one turn. Time: .66 min."

Our records show that all the trials subsequent to the first were without error.

Again our work loses part of its definiteness by our not knowing just what percentage of the loss in the rapidity of these first few trials after the operation is due to the process of 'forgetting.' (We thought we had provided for a control record by setting aside Rat. II., the 'anomaly' in the test on 'maze in darkness.' But it took us nearly 25 days to finish our experiments upon him, *i. e.*, to see whether he could learn to run the maze in the dark. Consequently, when the time came to test the blinded rats, rat II. was in perfect training.)

But even if these records were taken at their face value, we feel sure that our first point is made, viz., that when *once the normal rat learns the maze, the definiteness of his reactions to it* is little affected by the loss of vision. These results

from the experiments on the defective rats confirm those obtained by Dr. Carr and ourselves upon the normal trained rats with the maze in darkness. They add to those results, in that *here* any possible adaptation to darkness is impossible.

(*b*) *The Behavior of Blind Rats in Learning the Maze.*

There is still the possibility existing, that the rat may use vision in the *learning process.* While Dr. Carr, whose results have already been given, has gone a long way toward proving that such is *not* the case, by showing that normal rats can *learn* the maze as readily in the dark as in the light, there is a chance that adaptation to darkness (during the long early trials) might have entered in to aid the rats in making the records referred to. To prove that adaptation to darkness had no hand in the process of learning the maze in the dark, we removed the eyeballs from four *untrained* rats. The record of the recovery of these four rats together with their 'biography' has already been given upon page 48 of this paper. These four rats were not introduced to the maze until 42 days after the removal of their eyeballs. In Table VII. and its graphical representation, Curve VII, we show the phenomenal record made by these rats in learning the maze.

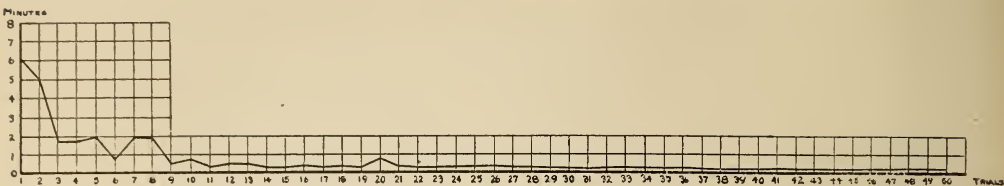
There is nothing to be gained by discussing these results in detail—the descriptions of the behavior of the normal rats in the maze already given by Small and ourselves sufficiently describe the method used by the blind rats in learning it. So far as we could judge, their behavior was absolutely normal. It is interesting to add, however, that the blind rat in learning the maze does not 'butt' into the cross pieces in the cul-de-sacs. He runs squarely down the middle of the galleries. He makes his turns into the various entrances as boldly and with as much sureness as do the nomal rats. The vibrissæ undoubtedly play a large part (though not an indispensable one) in the early reactions of these rats to the maze. But since the function of their vibrissæ is separately treated later on, we shall not dwell upon it here.

These experiments upon the untrained, blinded rats lend the needed control factor to Dr. Carr's experiments upon the

TABLE VII. Showing the average, minimum and maximum times of four blind rats in *learning* the maze.

No. of Trial.	Average. Minutes.	Minimum. Minutes.	Maximum. Minutes.
1	6.20	2.50	11.25
2	5.00	1.40	15.18
3	1.77	.93	3.53
4	1.78	.81	2.50
5	1.97	1.36	2.88
6	.73	.35	1.20
7	1.94	.71	4.93
8	1.89	.30	4.26
9	.52	.41	.63
10	.71	.25	1.38
11	.34	.25	.50
12	.57	.30	.73
13	.56	.25	1.00
14	.28	.23	.35
15	.24	.20	.26
16	.46	.25	.75
17	.24	.16	.30
18	.37	.18	.71
19	.32	.28	.41
20	.74	.30	1.85
21	.41	.25	.75
22	.26	.21	.35
23	.27	.25	.41
24	.36	.23	.53
25	.27	23	.31
26	.30	.23	.50
27	.27	.20	.41
28	.22	.16	.30
29	.23	.18	.33
30	.23	.18	.30
31	.19	.15	.23
32	.22	20	.25
33	.24	18	.33
34	.25	20	.30
35	.22	.18	.26
36	.31	.16	.65
37	.20	.16	.25
38	.18	.18	.20
39	.19	.16	.23
40	.19	.18	.25
41	.21	.18	.26
42	.20	.16	.26
43	.18	.18	.20
44	.19	.16	.21
45	.21	.18	.25
46	.20	18	.23
47	.21	.18	.30
48	.19	.18	.21
49	.20	.20	.20
50	.18	.16	.18

normal rats in learning the maze in the dark. They show, in our opinion, that his normal rats in thus learning the maze in the dark were not aided by any possible supra-human power of adaptation to darkness.



Curve VII. Constructed from Table VII.

(*c*) *The Behavior of Anosmic Rats in Learning the Maze.*

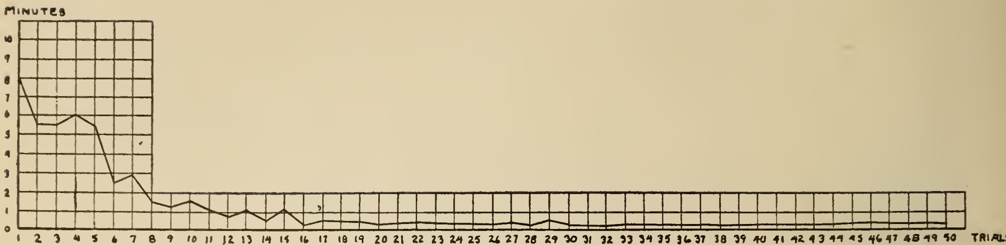
After having excluded vision, in our own opinion at least, as a necessary factor or even as an important auxiliary factor in the formation of the maze association, five rats, whose olfactory bulbs had been removed in the above described manner, were tried in the maze. Of these five rats, three were males and two were females. Two of the male rats were brothers of the four blind rats whose records we have just finished. The third male was a young rat, 150 days old. The two females were a little older but their exact ages were unknown. All five of the rats were in fine condition at the time of this experiment, but the two females were undoubtedly the finest rat specimens it has ever been our pleasure to examine. They were always active and curious whether hungry or well-fed.

Forty days were allowed to intervene between the operation and the first trial in the maze. All five rats were fed in the food-box of the maze for several days in the usual preliminary way before they were allowed to learn the maze. None of the rats had ever learned any problem prior to their experience with the maze. The records of these anosmic rats are shown in the following tables and curves (Tables VIII., IX. and X. and their corresponding curves). Table VIII. and its graphical representation, Curve VIII., show the averages of all five rats at each of the fifty trials. Table IX. and Curve IX. show separately the similar averages of the three male rats. Table X. and Curve X. show the corresponding separate averages for the two females.

TABLE VIII. Showing the average, minimum and maximum times of five anosmic rats in *learning* the maze.

No. of Trial.	Average. Minutes.	Minimum. Minutes.	Maximum. Minutes.
1	8.17	1.73	20.36
2	5.61	1.35	13.91
3	5.25	1.50	14.00
4	6.02	1.01	8.76
5	5.45	1.10	19.00
6	2.45	1.01	4.00
7	2.82	1.08	5.90
8	1.68	.88	2.75
9	1.35	.35	2.30
10	1.62	.35	5.20
11	1.03	.33	1.66
12	.71	.25	1.30
13	1.08	.20	2.66
14	.63	.25	1.08
15	1.14	.68	1.83
16	.43	.25	.63
17	.68	.58	.83
18	.51	.16	1.41
19	.56	.23	1.11
20	.34	.38	.55
21	.37	.20	.83
22	.48	.25	1.03
23	.37	.25	.43
24	.33	.16	.61
25	.33	.23	.50
26	.30	.15	.58
27	.44	.23	1.15
28	.26	.20	.41
29	.45	.20	1.31
30	.28	.18	.43
31	.29	.16	.58
32	.24	.16	.41
33	.30	.21	.50
34	.32	.20	.50
35	.28	.16	.63
36	.23	.15	.28
37	.33	.18	.71
38	.21	.15	.30
39	.22	.16	.28
40	.27	.20	.30
41	.25	.18	.33
42	.23	.18	.33
43	.21	.16	.26
44	.24	.18	.40
45	.23	.20	.30
46	.24	.18	.30
47	.21	.18	.23
48	.20	.16	.25
49	.21	.16	.26
50	.21	.16	.26

Strange as it may seem, we have little to add to what the bare records tell us in the way of comment upon the behavior of these anosmic rats in the maze. In short, their behavior



CURVE VIII. Constructed from Table VIII.

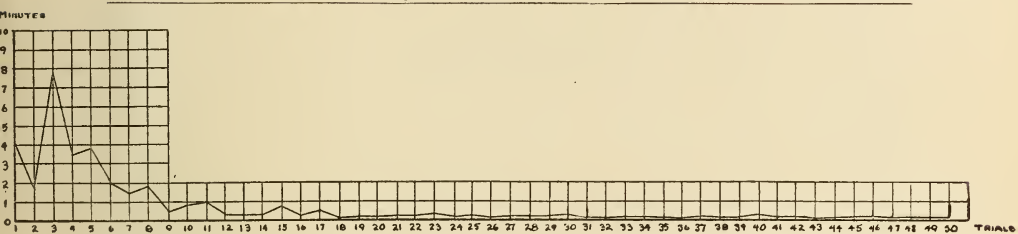
TABLE IX. Showing the averages of three anosmic males for learning the maze.

No. of Trial.	Average. Minutes	No. of Trial.	Average. Minutes.
1	10.88	26	.39
2	8.19	27	.57
3	3.58	28	.30
4	7.73	29	.62
5	7.86	30	.35
6	2.74	31	.39
7	3.68	32	.28
8	1.59	33	.36
9	1.48	34	.35
10	2.17	35	.35
11	1.05	36	.28
12	.17	37	.44
13	1.59	38	.23
14	.18	39	.25
15	1.38	40	.25
16	.53	41	.29
17	.72	42	.27
18	.73	43	.25
19	.78	44	.28
20	.44	45	.25
21	.46	46	.24
22	.62	47	.23
23	.39	48	.22
24	.43	49	.25
25	.36	50	.25

CURVE IX. Constructed from Table IX.

TABLE X. Showing the averages of two anosmic females for learning the maze.

No. of Trial.	Average. Minutes.	No. of Trial.	Average. Minutes.
1	4.10	26	.16
2	1.74	27	.24
3	7.75	28	.21
4	3.45	29	.20
5	3.82	30	.22
6	2.03	31	.16
7	1.53	32	.18
8	1.82	33	.23
9	.66	34	.28
10	.80	35	.18
11	.99	36	.15
12	.31	37	.22
13	.30	38	.18
14	.27	39	.18
15	.78	40	.29
16	.27	41	.19
17	.62	42	.19
18	.18	43	.16
19	.24	44	.19
20	.20	45	.20
21	.24	46	.24
22	.27	47	.19
23	.34	48	.18
24	.20	49	.17
25	.29	50	.16



CURVE X. Constructed from Table X.

in regard to the making of errors, the elimination of errors, etc., may be characterized as being normal. At first, we thought that the anosmic rats were going to take longer to get their 'cue' than the normal rats, but we found this to be true only for the first three or four trials. This failure to get the cue to the beginning of the series of movements the moment the anosmic rat is put down in the entrance is probably due to the lack of the customary olfactory sensations set up by the food which must act in the normal rats, at least in conjunction with the complex group of organic sensations (the hunger complex). When a normal rat is put down into the

entrance to the maze, as has already been mentioned, he usually loiters for an instant, then darts down the gallery. The inference is that the loitering is due to the swamping of the olfactory and organic neural impulses (which would ordinarily discharge into the motor area in the cortex, and thus condition the initial movement in the series) by the complex group of neural impulses (producing an 'emotion,' to phrase it in conscious terms) coming from the tactual, kinæsthetic and auditory stimulations incident upon the animal's introduction to the maze. The moment these subside, the neural impulses set up by the whiff of the food, coupled with those underlying the 'resurgence of the feeling of hunger' (Small) can discharge normally into the motor center. So far as we can see, there is no difficulty in this respect offered by the anosmic rats. All we need assume is that here the organic complex alone can discharge into the proper motor center and by so doing release the movement which formerly had been released by both sets of afferent impulses.

Many tests were made to prove that these rats were really anosmic. Summarizing them briefly, we may say that these rats had to learn to get their food by the 'sense of position' (sight does not seem to aid them). This was true even of cheese, which might conceivably affect the N. Trigeminus. But the 'sense of position' is so acute, that one who is not thoroughly familiar with it would be tempted to deny that the rats are anosmic. What especially gives them the appearance of smelling is the fact that the movements produced in 'sniffing' and the movements of the vibrissæ are still present after the operation. That these are largely tactual reflexes is shown by the fact that they still persist even after a large part of both of the frontal lobes is removed. (Two animals were thus operated upon.)

One characteristic test will be given in detail: Undiluted oil of cassia was put down on the floor of the gallery just around the corner marked *r* in our diagram. This was allowed to dry thoroughly so that the contact conditions would not be altered. The following description of the behavior of the

blind and the anosmic rats to this disturbing element in the maze is taken from our diary:

Blind Rat I.	1st trial.	Stopped .13 min. at the cassia. Total time: .36 min.
Blind Rat I.	2d trial.	Stopped .25 min. *Decided not to pass it and returned home,* running into the first cul-de-sac (something he has not done for weeks). Taking up his cue again, he started to the food-box but stopped again at the cassia for .08 min., then passed it and went to food. Total time: .83 min.
Blind Rat II.	1st trial.	Paused at cassia but did not linger. Time: .20 min.
Blind Rat II.	2d trial.	Paused at cassia but did not linger. Time: .20 min.
Blind Rat III.	1st trial.	Came to dead stop at cassia and waited there .08 min. Total time: .26 min.
Blind Rat III.	2d trial.	Stopped for .08 min. at cassia. Turned and went home. On return went into the cul-de-sac, emerging he turned corner at *r* but again could not stomach cassia. Turned into cul-de-sac *B* and apparently was utterly confused. After this he made several trips between *O* and cassia. Again and again he stuck his nose into the turn smelling of the cassia, but he would not pass it. At the end of 9 min. he was taken out and petted and later was tried in the maze. He could not be prevailed upon to pass the cassia. It was several days—although we had assiduously scoured the maze after the rat's first day's trial—before he finally crossed the place smelling of the cassia.
Blind Rat IV.	1st trial.	Paused at cassia and sniffed but did not delay his journey to the food-box. Time: .23 min.
Blind Rat IV.	2d trial.	Repeated the behavior of first trial. Time: .25 min.
Anosmic Rat *Y*.	1st trial.	Passed like a streak over cassia. Time: .20 min.
Anosmic Rat *Y*.	2d trial.	Passed like a streak over cassia. Time: .16 min.
Anosmic Rat *Z*.	1st trial.	No sign of noticing cassia. Time: .21 min.
Anosmic Rat *Z*.	2d trial.	No sign of noticing cassia. Time: .18 min.
Anosmic Rat *A*.	1st trial.	Did not notice cassia. Time: .23 min.
Anosmic Rat *A*.	2d trial.	Did not notice cassia. Time: .18 min.
Anosmic Rat *B*.	1st trial.	Did not notice cassia. Time: .20 min.
Anosmic Rat *B*.	2d trial.	Did not notice cassia. Time: .20 min.
Anosmic Rat *D*.	1st trial.	Was not hungry so could not get him to go to food, but in his 'loafing' *he passed and repassed cassia many times.* At no time did he show any signs of noticing it.

We think that the above tests are striking enough to convince any one that these five rats did not smell. We, ourselves, after viewing the operation to remove the *bulbi olfactorii* were

convinced that if the rat ever smelled again, either of two facts would have to be assumed: First, that central regeneration had taken place; or, second, that the N. Trigeminus functions as an olfactory structure. In regard to the first point, we shall say that histological examinations now going on will show whether or not any central regeneration has taken place, or is in the process of taking place in these rats. There is no evidence gathered from our experiments which tends to show that these rats can smell, consequently we have no ground for assuming *a priori* either that regeneration has taken place or that the N. Trigeminus functions as a specific olfactory structure.[1]

Before concluding this discussion of the learning process of the anosmic rats, it remains to be noted that we did not (as was the case with the blind rats) remove the *bulbi olfactorii* from rats already trained to the maze. We justified ourselves for taking this position by assuming that if the anosmic rats could *learn* the maze *as rapidly* as the normal rats, no experimental evidence would be needed to support the inference that the rats could still run the maze if they were forced to lose their sense of smell.

(*d*) *The Behavior of Anosmic Rats Trained to the Maze in the Light, when Introduced to the Maze in Darkness.*

The five anosmic rats, after having had their fifty trials in the light, were tried five times each in the maze with the light excluded (see conditions of this test, page 36). We present below the five records of each rat made in the dark with a comparison set of records of each rat made 4 hours previously in the light.

With the one exception the records in the dark are practically equal in point of time to their companions in the light. There is a slight tendency for the records in the dark to average a little higher than those in the light but whether this is due to accident or whether it marks a real difference in behavior cannot be decided from the limited number of records. The possibility is at hand, that the shutting off of the function of two sense organs tends to decrease the activity of the animal.

[1] To settle this point absolutely, however, would take extended observations.

	In Light.		In Darkness.	
	Trial.	Min.	Trial.	Min.
Rat *Y.*	1	.18	1	.26
	2	.18	2	.16
	3	.16	3	.16
	4	.16	4	.16
	5	.16	5	.25
Rat *Z.*	1	.30	1	.25
	2	.20	2	.23
	3	.20	3	.28
	4	.18	4	.21
	5	.16	5	.18
Rat *A.*	1	.25	1	.30
	2	.23	2	.33
	3	.21	3	.25
	4	.26	4	.33
	5	.26	5	.26
Rat *B.*	1	.23	1	.25
	2	.23	2 } Slight confusion in running the first half of the maze.	.41
	3	.25	3 }	.20
	4	.26	4	.20
	5	.25	5	.21
Rat *D.*	1 2 3 4 5	.25 .23 .20 .23 .25	Repeated the behavior of normal Rat II. Numerous tests were taken. He simply could not run the maze in the dark. Every time the room light was turned on he ran to the food-box. He likewise could run the maze when the miniature lights were caused to glow faintly. Detailed experiments were made, but the results are not presented because they are identical with those obtained from experiments upon normal Rat II. (see page 42). (This rat had developed a bad cough similar to Rat II.)	

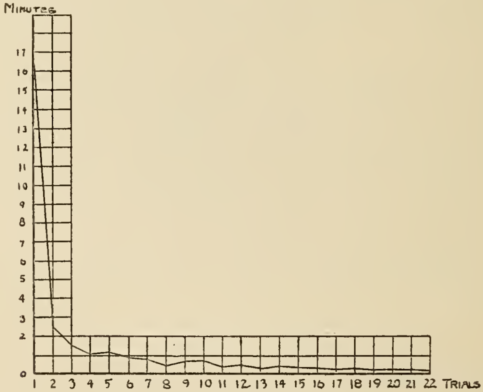
(*e*) *The Behavior of* (*Partially*) *Deaf Rats in Learning the Maze.*

The two young females whose middle ear apparatus had been thrown out of function and whose period of convalescence has already been described, were next tried in the maze. Only eight days were allowed to elapse between the operation and the beginning of the trials in the maze. We feared that these animals might in some way remove the paraffine from the middle ear cavity and that a new tympanic membrane (at least one composed of scar tissue) might develop. Forcing the rats to learn the maze eight days after the operation in all probability removes such a contingency.

The rats suffered so few ill consequences from the operation, that we felt that their records could be relied upon. These two females, as has been stated above, were about 150 days old. They were exceedingly active. Below (Table XI, Curve XI.) we give the usual records of their trials in learning the maze.

TABLE XI. Showing the averages of two young (partially) deaf females in learning the maze.

No. of Trial.	Average. Minutes.	No. of Trial.	Average. Minutes.
1	16.53	12	.44
2	2.50	13	.23
3	1.48	14	.48
4	1.18	15	.28
5	1.20	16	.38
6	.84	17	.28
7	.70	18	.25
8	.38	19	.20
9	.61	20	.25
10	.78	21	.25
11	.34	22	.20



CURVE XI. Constructed from Table XI.

As the records show, the behavior of these rats was perfectly normal. In fact, one of them undoubtedly 'holds the record' in this laboratory for excellence in learning the maze. No peculiarity was noticeable in their behavior when passing from a long gallery into a short one and *vice versa*, or when passing entrances, etc., as one might expect would be the case

if the sensitivity of the tympana to changes in the pressure of the air columns is at all responsible for the formation of this association. At the end of this experiment the aural sensitivity of these rats was still markedly below normal.

IV. *Experiments Designed to Test the Function of Cutaneous Sensations in the Formation of the Maze Association.*

1. *The Effect of the Removal of the Vibrissæ from Trained Normal and Defective Rats.*

The vibrissæ of the white rat are long and numerous and exceedingly mobile—very similar to those of the cat except that the latter's are stationary. The rat's vibrissæ are in constant motion. Below, we present two drawings of these vibrissæ—one, the view from beneath the head, the other looking down from above. Especial attention is called to the fact that they project from the nose of the rat in five tiers. The vibrissæ nearest the anterior nares are most mobile.

On account of the extreme mobility of these hairs, we decided *a priori* that they must be of extreme usefulness to the rat in making his proximate orientation in the maze. We did not see how it was possible for them to serve as the basis for making the turns—*i. e.,* in any discriminatory way—but we felt that the vibrissæ would be indispensable to him in detecting an entrance or a wall (*i. e.,* at least to the blind animal). It is easily demonstrated by any one that the rat in learning the maze does not run into the cross pieces in the maze ' head on '—the vibrissæ undoubtedly warn him of the presence of solid objects. But when a labyrinth path is learned and the rat begins to traverse it with assurance, he will sometimes run head on into an obstruction placed in his way. This behavior suggests, to some extent at least that the function of the vibrissæ may be dispensed with once the pathway is thoroughly learned. A number of tests were made. As a preliminary to this test, we ran our normal, anosmic and blind rats around the maze for several days—until their time was minimum and constant from day to day. When they were in thorough training, with a reaction time for traversing the

maze from .20–.30 min., we proceeded as follows: First, in order to make sure that the rats were hungry, and that their reactions for that day could be trusted, two normal tests were

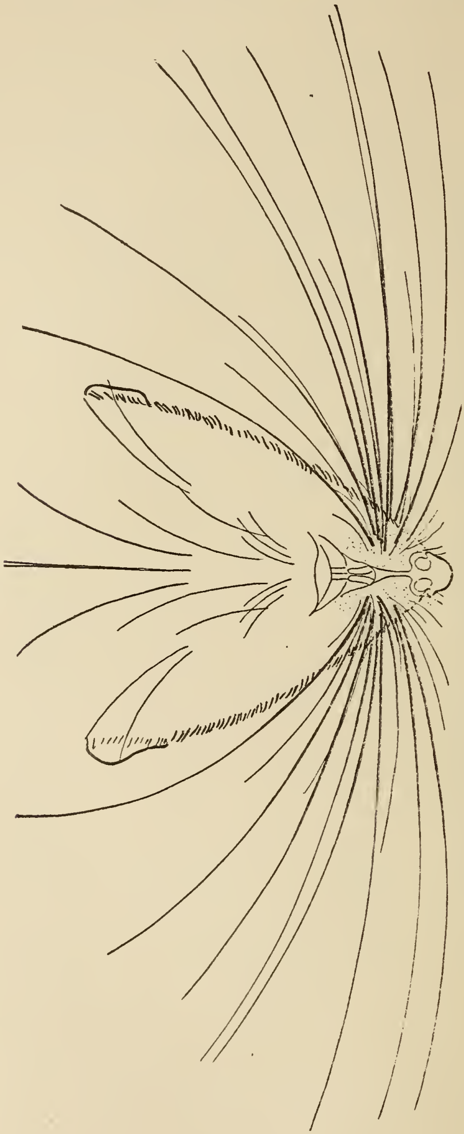


Fig. 3. Showing Vibrissæ of the White Rat—viewed from below. (Life size.)

given each rat before the vibrissæ were cut off. After the two normal tests had been given, the vibrissæ were closely cut

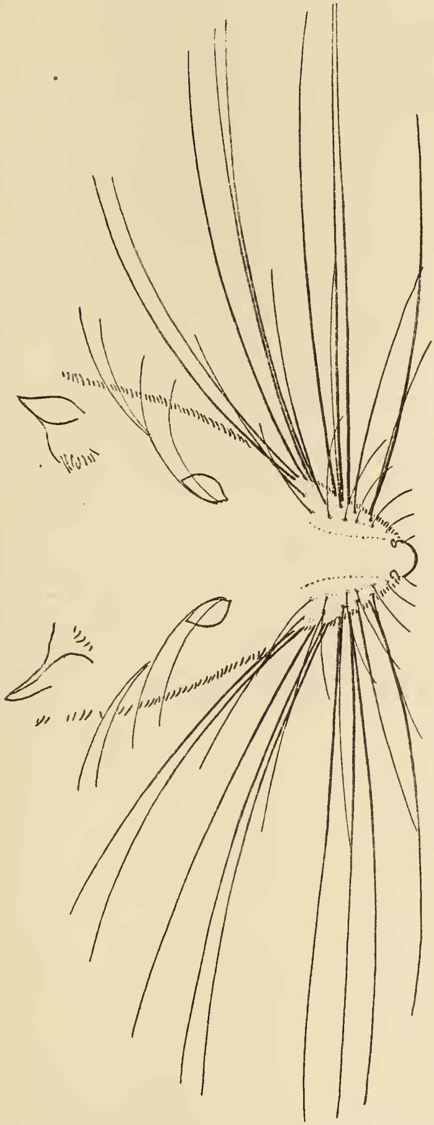


FIG. 4. Showing Vibrissæ of the White Rat—viewed from above. (Life size.)

off. The rats were then immediately tried in the maze. The results of this test are presented below.

RECORD OF TWO BLIND RATS.

Rat I. 1st normal trial. Time: .25 min.

Rat I. 2d normal trial. Time: .25 min.

Rat I. 1st trial. After removal of vibrissæ. Bumped into sides. Ran into cul-de-sacs *A* and *B*. Went head on into nearly every corner. Hugged sides of galleries. Walked very slowly. Gave apparent signs of affective disturbance after butting into walls. Time: 1.28 min.

2d trial. Hugged sides of galleries. Walked slowly. Made no errors of turn. Time: 1.08 min.

3d trial. Ran into *A*. Bumped into walls but less than before. Gathered speed after center of maze was passed. Time: .50 min.

4th trial. Much improvement. No longer creeps. Does not butt into walls. Error at one place. Time: .43 min.

Rat II. 1st normal trial. Time: .25 min.

Rat II. 2d normal trial. Time: .25 min.

Rat II. 1st trial. After removal of vibrissæ. Duplicated behavior of Rat I. Time: 1.40 min.

2d trial. Very great improvement. No errors and no hugging of sides. Slight hesitancies. Time: .41 min.

3d trial. Further improvement. Time: .36 min.

4th trial. Further improvement. Time: .35 min.

Twenty-four hours later, these two rats were tried again in the maze. The reactions of the rats at that time were practically normal. After the first two trials, no sign of disturbance was noticeable.

Two anosmic rats were next tried under exactly similar conditions.

RECORD OF TWO ANOSMIC RATS.

Rat I. 1st normal trial. Time: .26 min.

Rat I. 2d normal trial. Time: .25 min.

Rat I. 1st trial. After removal of vibrissæ. Walks slowly. Butts into cross pieces fully as much as the blind rats did. Errors in turns. Gathers speed after the center of maze is passed. Time: 1.28 min.

2d trial. "Lets herself go." No error, but butted full into wall. Time: .56 min.

3d trial. Improvement. Time: .55 min.

4th trial. Improvement. Time: .51 min.

Rat II. 1st normal trial. Time: .21 min.

Rat II. 2d normal trial. Time: .20 min.

Rat II. 1st trial. After removal of vibrissæ. Butted full into wall at first turn. Picked herself up and fairly flew for the rest of the way. Time: .30 min.

2d trial. A little slow but steered herself through entrances and down center of galleries with all ease. Time: .35 min.

3d trial. We allowed her to eat too much on 2d trial and the edge had been taken from her appetite. Time: .50 min.

Twenty-four hours later, these rats gave normal reaction times—Rat II. going below her normal time as given above (1st, .20 min.; 2d, .23 min.; 3d, 18 min.; are her records on the second day's trial).

Record of Two Normal Rats.

Rat I. 1st normal trial. Time: .21 min.
Rat I. 2d normal trial. Time: .23 min.
Rat I. 1st trial. After removal of vibrissæ. Butted square into walls. Ran into cul-de-sacs. Hugged sides of galleries. Ran with jerks and starts. Clung to floor with feet (a very ludicrous bit of behavior). Time: .38 min.
2d trial. Similar behavior. Time: .38 min.
3d trial. No error. Hugged sides of galleries. Time: .23 min.
4th trial. "Let himself go." Time: .23 min.
Rat II. 1st normal trial. Time: .17 min.
Rat II. 2d normal trial. Time: .16 min.
Rat II. 1st trial. After removal of vibrissæ. Clung to floor. Error in turn and ran full length of one cul-de-sac. Peculiar jumping movements. Time: .45 min.
2d trial. After removal of vibrissæ. Reaction rapid but jerky. Time: .25 min.
3d trial. Perfect. Time: .21 min.
4th trial. Perfect. Time: .21 min.

The effect of the removal of the vibrissæ, even under the above conditions, viz., the removal of the vibrissæ and the immediate trial in the maze, was so transient, that we thought that by varying the conditions of the experiment somewhat we might eliminate any disturbance of the reactions. It is obviously unfair to the animal to try him immediately after removing the vibrissæ. The effect upon the emotional condition of the animal must at first be very marked. We ourselves feel 'queer' when our finger nails are cut to the quick or when our hair has been closely cut—the effect is noticed more on the affective side than on the cognitive. After a day or two, however, our organism adapts itself to the changed condition and we cease to notice the 'feel' of the closely cut hair or nails.

Thinking that a similar process of adaptation might go on in the case of the rat, we adopted the following procedure upon four trained rats—two normal rats and two partially deaf rats (whose records in learning the maze have been given): On a given day, five normal reactions were taken. The vibrissæ were then removed and the rat put back into his cage. 48 hours later (rats having been fed once in the food-

box of the maze) the rats were allowed to run the maze. Below, we give respectively the five normal records and the records 48 hours after the removal of the vibrissæ.

TESTS ON NORMAL RATS.

Normal Trials.

Rat I.	1st trial.		Time: .23 min.
Rat I.	2d trial.		Time: .20 min.
Rat I.	3d trial.		Time: .21 min.
Rat I.	4th trial.		Time: .21 min.
Rat I.	5th trial.		Time: .23 min.

Tests After Removal of Vibrissæ.

Rat I.	1st trial.	Perfect.	Time: .23 min.
Rat I.	2d trial.	Perfect.	Time: .25 min.
Rat I.	3d trial.	Perfect.	Time: .26 min.

In all three trials this rat struck out into the open. There was no confusion, no hesitancy and no butting into walls.

Normal Trials.

Rat II.	1st trial.		Time: .25 min.
Rat II.	2d trial.		Time: .18 min.
Rat II.	3d trial.		Time: .18 min.
Rat II.	4th trial.		Time: .18 min.
Rat II.	5th trial.		Time: .20 min.

Records After Removal of Vibrissæ.

Rat II.	1st trial.	Perfect.	Time: .25 min.
Rat II.	2d trial.	Perfect.	Time: .21 min.
Rat II.	3d trial.	Perfect.	Time: .23 min.

TEST ON TWO DEAF RATS.

Normal Records.

Rat I.	1st trial.		Time: .26 min.
Rat I.	2d trial.		Time: .31 min.
Rat I.	3d trial.		Time: .25 min.
Rat I.	4th trial.		Time: .41 min.
Rat I.	5th trial.		Time: .36 min.

(Her normal reactions were usually slightly higher than the average.)

Records After Removal of Vibrissæ.

Rat I.	1st trial.		Time: .33 min.
Rat I.	2d trial.		Time: .33 min.
Rat I.	3d trial.		Time: .31 min.
Rat I.	4th trial.		Time: .33 min.

Her behavior was perfectly normal in every respect. There was no clinging to the sides of the galleries nor creeping around corners.

Normal Records.

Rat II.	1st trial.	Time: .20 min.
Rat II.	2d trial.	Time: .20 min.
Rat II.	3d trial.	Time: .20 min.
Rat II.	4th trial.	Time: .20 min.
Rat II.	5th trial.	Time: .23 min.

(This rat won the prize for excellence in *learning* the maze.)

Records After Removal of Vibrissæ.

Rat II.	1st trial.	Time: .18 min.
Rat II.	2d trial.	Time: .20 min.
Rat II.	3d trial.	Time: .16 min.
Rat II.	4th trial.	Time: .20 min.

From these experiments upon the removal of the vibrissæ, we think it safe to say, first, that under normal conditions the rat probably uses his vibrissæ to assist him in making proximate orientation in the maze;[1] second, in all probability the rat does not discriminate his turns by means of any data contributed by the vibrissæ; third, the immediate effect of the loss of the vibrissæ upon the 'steadiness' of the rat's reactions in the maze is only transitory—the steadiness of the reaction being not at all affected if the rat is allowed to habituate himself to the environment of his living cage after the removal of the vibrissæ.

2. *The Process of Learning the Maze Without Vibrissæ.*

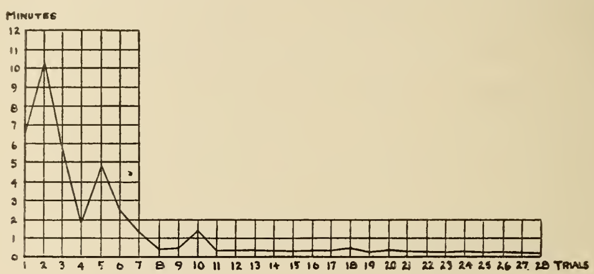
Professor J. M. Baldwin, who was present at several of the above trials, suggested that we allow two rats with vibrissæ removed to learn the maze. In the following table (Table XII.) and its graphical representation (Curve XII.), the average records of two female rats (age, four months) are shown.

As may be surmised from the foregoing curve and table, their behavior was quite normal.

[1] Be it understood, however, that we have no crucial positive evidence in support of this assumption. The evidence as it stands here proves unquestionably that the animal is not disturbed in his reactions by the removel of the vibrissæ if time is given him to wear off the unaccustomed 'feel of their absence'—but it does not give unequivocal positive evidence that they are even normally used by the rat in sensing openings, walls, etc.

TABLE XII. Showing the average time of two female rats whose vibrissæ had been removed in learning the maze.

No. of Trial.	Average. Minutes.	No. of Trial.	Average. Minutes.
1	6.89	15	.30
2	10.35	16	.35
3	5.75	17	.29
4	1.70	18	.54
5	4.68	19	.23
6	2.45	20	.35
7	1.25	21	.20
8	.45	22	.20
9	.50	23	.19
10	1.44	24	.24
11	.30	25	.20
12	.35	26	.20
13	.30	27	.19
14	.29	28	.20



CURVE XII. Constructed from Table XII.

3. *The effect of Altering the Temperature Conditions in the Maze.*

The question at issue under this heading is not, can the rat learn to associate a change of temperature with a certain movement of the body, but it is rather a question of existing fact—does he, as he traverses the maze, under unmodified conditions, utilize the possible differences in the temperature values to be found at the correct turns *versus* the incorrect? Such differences must be very slight if they exist at all. Thinking, however, that in addition to the slight absolute differences in the temperature of the various parts of the maze, that the correct turns as over against the incorrect might offer a difference in the reflection of the heat of the animal's own body (in the sense of 'facial vision') we decided to make a test case. At the point marked 2 in the maze, we inserted a four inch

square copper plate (we had arranged for several such copper plates at the points marked 2, *a,* 6, *b* in the maze). The trained rats were allowed to run by this plate on their regular daily trips until they were accustomed to it. As a test, we cooled the copper plate to the *freezing point* and allowed two anosmic rats, two normal rats and three blind rats to run the maze. Every rat 'kited' by the copper plate with no sign of disturbance in his time records. The cold plate was then replaced by a hot one (approximately 75° C.). All of the rats were again tried. No sign of disturbance was noted. (Three observers were present at this test and all agreed that in no case was there any sign of disturbance.)

4. *The Effect of Changing the Direction of the Air Currents in the Maze.*

For this purpose a large electric fan was used. A tiny flag, placed on the cover of the maze immediately above the entrance at *r,* showed at any time the direction of the air currents. The fan was first placed to the south of the maze. A heavy current of air was thus driven through the maze in a northerly direction (this is strictly true only of the galleries which run north and south). Before being tried in the maze, each rat was accustomed to the noise of the fan and to the heavy current of air by allowing him to run freely around the outside cover of the maze while the fan was in action. Each rat (three blind, two anosmic, two deaf and one normal) was given two trials with the air currents flowing from W.-E. Three observers watched the behavior of the rats—all agreed that it was perfectly normal in every respect. *No errors were made and in no case was the usual time of the trip lengthened.*

5. *The Effect of Local Anæsthesia.*

Ethyl-chloride was the anæsthetic used in the following experiments. It was used in preference to cocaine for the reason that cocaine, when applied in amounts sufficient to cause tactual anæsthesia, affects the musculature as well.

The first attempt to isolate the cutaneous impression from the kinæsthetic by the above method was made by anæsthetiz-

ing the soles of the feet of two blind rats. The ethyl-chloride was applied to the soles of the feet of these two rats three times at intervals of three to four minutes. After the last application, they were immediately put down in the entrance to the maze. The following records show that the reactions of the rat were not disturbed.

RAT I.

1st trial. "Feet made anæsthetic. This had absolutely no effect." Time: .18 min.

2d trial. "Reanæsthetized the soles of the feet with the same result." Time: .23 min.

RAT II.

1st trial. "Absolutely perfect." Time: .20 min.

2d trial. "Stopped in maze to bite his feet. No error." Time: .30 min.

One other test was made by anæsthetizing the nose of a trained anosmic rat. Her several successive reactions were not in the least disturbed, although the ethyl-chloride was very thoroughly applied.

Still another similar test, the result of which was likewise *nil,* was made. We applied a heavy coating of collodion to the anterior (bare) portion of the snout of a trained blind rat. Twenty-four hours later this rat was tried in the maze. His reactions were normal.

Summarizing the results of these necessarily inexact attempts to separate the cutaneous impressions from the kinæsthetic, we may say, briefly, that the indications point to the fact that the rat in no way uses his cutaneous sensations as a basis for 'sensing' the correct turns in the maze as distinguished from the incorrect.

V. *Experiments to Determine the Delicacy of the Sense of Taste in the White Rat (viz., the Affective and not the Cognitive Reaction).*

In carrying out these experiments upon the maze, we were repeatedly asked the question, "How delicate is the sense of taste in rats?" Some hardily inclined visitors even went so far as to suggest that the sense of taste might aid the rat in making the correct turns in the maze! While having little

faith in the notion that the rat 'tastes' his way around, still, in view of the fact that some general knowledge concerning the delicacy of taste in rats might possibly be of value in later investigations, we began experimentation in this field.

We finally adopted the following method, after many attempts to find a better one: Aqueous solutions, differing widely in percentages, were made of the four usual taste substances, salt, sugar (cane), hydrochloric quinine and tartaric acid. Small cubes of baker's bread saturated in these solutions were presented to the rats. At first the bread was given in the food-box of the maze, but it was quickly found that the rats would accept the bread saturated with stronger solutions if it were presented to them in their living cages. A small clean dish was placed in the cage to receive the bread. This was washed after every test. Likewise the experimenter's hands were washed after each experiment. The rats were kept uniformly hungry. Only three or four tests were given each day. After the day's experiments were over the rats were fed with their usual amount of food. Starting the tests for the day with one set of the different percentages of one of the taste solutions, we presented first the weakest solution, then the next stronger, etc., until the solution became so strong that the rat rejected it. The percentage of the solution definitely rejected was noted and the next day the rat was given this percentage at the beginning of the experiment. Since it would be impossible for this paper to present the results of all the tests made with the four taste solutions, we will give from our notes a complete account of only one set, and summarize the remainder.

6/1/06.

QUININE SOLUTIONS.

Rat A (*Anosmic*).

.01 per cent. quinine.	Accepted.
.05 per cent. quinine.	Accepted.
.1 per cent. quinine.	Ate small portion and then definitely rejected it.

Rat B (*Anosmic*).

.01 per cent. quinine.	Did not seem to relish it. Ate it nevertheless.
.05 per cent. quinine.	Ate it very slowly.
.1 per cent. quinine.	Rejected it.

Rat D (Anosmic).

.01 per cent. quinine. Ate it very slowly.
.05 per cent. quinine. Rejected it after taking a small amount.
.1 per cent. quinine. Rejected it.

Rat Y (Anosmic).

.01 per cent. quinine. Accepted.
.05 per cent. quinine. Ate it very slowly.
.1 per cent. quinine. Rejected.

Rat Z (Anosmic).

.01 per cent. quinine. Accepted.
.05 per cent. quinine. Accepted, ate part, left it, returned and finished eating it.
.1 per cent. quinine. Rejected.

Four Blind Rats.

All ate the .01 per cent. and the .05 per cent. solutions. Rats II. and III. actually ate a small portion of the .1 per cent. solution.

6/2/06.

Rat A (Anosmic).

.1 per cent. quinine. Tasted it and rejected it but came back and ate one-third.

Rat B (Anosmic).

.1 per cent. quinine. Tasted it and turned away. Came back, tasted and finally rejected it.

Rat D (Anosmic).

.1 per cent. quinine. Ate small portion and rejected rest.

Rat Y (Anosmic).

.1 per cent. quinine. Ate nearly one-half of it.

Rat Z (Anosmic).

.1 per cent. quinine. Rolled bread over with paw after tasting. Finally ate one-half.

Blind Rats.

All ate a part of the bread. They would take the morsel, drop it, pick it up again and again drop it. After a time, interest in the food was lost. Finally they came back to it and consumed a very small portion. At the end of one hour, the four small pieces were not completely consumed.

Normal rats were tried in the same way and with identical results. The question may be asked how we came to select the .01 per cent., .05 per cent. and .1 per cent. solution of quinine. Our answer is, that all solutions weaker than these were wholly unnoticed. We began the experiments by making solutions near the human cognitive threshold, but it was quickly found that humanly speaking the rats affective reaction to

bitter is a very gross affair. Certainly, we should have to be almost frantic with hunger before we should eat food soaked even in a .01 per cent. solution of quinine. Particular pains were taken to see that the rats were not frantic for food. Many tests were made with rats whose appetite had lost its edge through partial feeding—even under this condition, they would often accept the food soaked in .05 per cent. solutions and always that soaked in the .01 per cent.

Summarizing the remainder of the results upon these taste experiments, we find:

1. Solutions of cane sugar—at least up to 80 per cent.—are accepted eagerly.

2. Tartaric solutions are accepted in most cases up to 1 per cent. Rejection begins at .5 per cent. when passing from weak solutions to stronger; when tested at the beginning of the day's experimentation with a 1 per cent. solution, a small quantity of it is sometimes taken.

3. Salt solutions are accepted without question up to 5 per cent. Rejection is apparent in a large majority of the cases at this point. A few cases were noticed where a small amount of the 10 per cent. solution was taken.

If we may be allowed to draw any conclusions whatever from these crude and inexact tests upon the taste senstitivity of the rat, we should say that the indications are that the sense of taste in these animals is far too blunt to give any sensory data which could guide them in their journey through the maze.

VI. *Some Preliminary Experiments Bearing more or less Directly upon the Present Problem.*

1. *Experiments Designed to Test how Readily the Rat can Orient Himself when Put Down at Various Places in the Maze.*

In learning the maze, as has already been mentioned, the rats often return to the starting point for a new 'cue' after having gone a certain distance toward the food-box. Gradually this tendency disappears and the rat becomes more and more competent to pick up the true pathway even after com-

ing out of a cul-de-sac. What happens when a perfectly trained animal is put down at such places as those marked X_1, X_2, X_3?[1] Has he the power to pick up the true pathway immediately at any point in the maze—no matter whether a forward or backward orientation has been given his body? Or must the animal go backward or forward around some corner or corners before he 'senses' his position in the maze? To take a specific example: Suppose a rat to be put down with wrong orientation at X_1. Will he turn immediately and scamper for the food or will he go on around *s* and *t* before turning and proceeding in the right direction?

To answer these and similar questions, some two hundred trials were made. Of these trials, 98 were made at the point marked X_1—the rest were made at X_2 and X_3. Our method was as follows: The rat was given a preliminary 'run' to freshen up his 'knowledge' of the maze. He was then put down at X_1 with his head pointed in the right direction (toward the food-box) and his efforts to orient himself were noted. At the next trial he was put down with his head pointed in the wrong direction (towards entrance) and similar records were made of the process of orientation. The tests are most satisfactory if the animal is allowed to have a small bit of food in his mouth when he is put down in the maze. He stops to eat this before beginning the act of orientation. This gives the experimenter time to close down the cover of the maze and to withdraw. The first question which comes to one's mind in such a test as the above is this, has the rat a natural tendency to turn around when put down in a gallery? We cannot answer this with certainty but our experiments enable us to state that in the 98 trials given in X_1, the animal went straight ahead in 47 cases and turned in 51 cases, regardless of the orientation given his body. On the whole we should say then that the tendency to turn and the tendency to go straight ahead are about equal.

Coming to the facts of orientation, we find: First, that in 59 cases out of the 98 (60.2 per cent.) given at X_1, the orientation was immediate. That is, if the animal was put down

[1] See cut of maze, p. 10.

with wrong orientation he turned immediately and went to the food box; if put down in the maze with right orientation, he went straight forward. But in these 59 cases, in addition to those already mentioned, we have included all those cases where an animal, put down with right orientation, turned 360° before starting to the food-box (about 12 per cent. of the total number of the cases of right orientation) and all those cases where an animal, put down with either right or wrong orientation, picked up his 'cue' somewhere on the straightaway at X_1, *i. e.*, picked up the pathway without having to turn a corner.

To explain this a little further, let us suppose a rat to be put down at X_1 with wrong orientation; sometimes he would proceed with that orientation for only 15–20 cm., then suddenly, before turning the corner at *s*, he would turn at 180° and scamper for the food-box. Now suppose that we put him down at X_1 with right orientation; he would sometimes turn 180° and go back for 15–20 cm., but before turning the corner at *s*, he likewise would turn again 180° and scamper for the food. Second, in 38 cases out of the 98 (39.8 per cent.), the orientation was made only after turning corners. If the animal was put down at X_1 with wrong orientation, he would proceed on around *s* and *r*, continuing often into one of the cul-de-sacs *A* or *B*; emerging from either of these, he would pick up the true pathway and dart off immediately. If put down with right orientation, he would turn and behave exactly as in the preceding case. In a few of the cases, the animal rightly oriented would proceed with that orientation around *t* and *u*, he would then turn and go back, possibly to *t* or *s* before again turning for the last time. In 7 of the cases, the rat went completely back to the entrance before getting his 'cue.' The singular part of it all is, that when the animal gets his cue in any part of the maze, he gets it just as definitely there as at the entrance. Very rarely was an error made after the animal became oriented, *i. e.*, after he had started off 'hard.'

So far as our records go, they show that the blind and

anosmic rats oriented themselves fully as well as the normal animals.

The results of the tests made at X_2 and X_3 are very similar to those at X_1.

It would be interesting to know in those cases where orientation did not take place immediately whether it was necessary for the rat to turn the corners in order to get his bearings. This question cannot be adequately answered by experiments upon our maze—the straightaways are far too short.[1] If the animal moves either forwards or backwards for any considerable distance, he must inevitably turn a corner. While a large number of the tests show that the rats do make the turns before obtaining the correct orientation, we are not justified in assuming that the turning *per se* gives the cue to orientation. In the near future we hope to construct a maze having a six-foot straightaway somewhere near the center of the course. Under such conditions, it may turn out that the rat can orient himself without rounding corners. The fact that so many of the above orientations were made 'immediately' lends some support to the view that the act of turning *per se* is not a *conditio sine qua non* to the process of orientation. (There is, as everyone can see, a large source of possible error in our percentage of right orientation. We have not excluded the possibility of the animal's *learning to turn, i. e.,* irrespective of the 'sensing' of his position in the maze. There is no reason to suppose that such an association would not be established provided the animals were given a sufficient number of trials. In the above tests, the animals were given about 12 trials each.)

Until the facts of orientation in the maze are more clearly established, we feel sure that it would be futile to discuss the 'control factors' by means of which the rat 'finds' his position in the various parts of the maze. In view of the fact, however, that we shall take the position in this paper that the kinæsthetic impressions coupled with certain other intra-organic impressions are the only necessary sensory factors used in the

[1] Nevertheless a complete answer will have to be returned to this question before we can advance any but a very tentative theory of how the rat 'controls' his kinæsthetic sensation series.

formation of the maze association, it remains to be said (in our opinion at least), that the above results concerning the facts about orientation offer no insurmountable difficulties to such a view.

2. *The Effect of Rotating the Maze.*

After the failure of our attempts to disturb the reactions of the rat by modifying the conditions in the maze, we were ill prepared to find that the simple rotation of the maze 90°, 180°, etc., would have serious consequences upon his behavior. We probably should have been prepared for such consequences, however, because the so-called 'sense of position' ('sense of direction'?) in this animal is extremely well marked.[1] Small has already mentioned this fact and we have had many occasions to observe it anew in our further work with these animals. The facts come out quite clearly in the behavior of a rat when at work upon a problem box such as that described in *Animal Education* (p. 13), where the first step in obtaining the food consists in scratching away the sawdust from one end of an oblong box. If, after the animal has once or twice successfully obtained food by entering at a given end of the box, the box now be rotated, *e. g.*, 45° or 90°, the animal becomes confused and is likely to scratch at the former position of the end of the box. It takes him some little time to adapt himself to this simple change in the orientation of the box. Again, a curious disturbance of the same kind was noticed in some of Mr. Peterson's rats. He was testing the rats with a small problem box having a door in one side which was held in place by a latch. A spring opened the door of the box when the

[1] If this sense of direction should be established by further tests, its experimental demonstration should date back to Bethe's work upon the rotation of the bee-hive. For a short review of his tests, see the review of the Peckham's book, *Wasps, Social and Solitary,* Psychological Bulletin, Vol. III., No. 5, p. 172. Many other references might be given to the legions of articles on the flight of pigeons, etc., but none of the experiments cited in them in support of a sense of direction is controlled in any proper sense. Porter's experiments on the reversed maze (Am. Jr. of Psy., Vol. 17, p. 256) are open to the objection that the environment was not 'reversed' at the time of the reversion of his maze. Bethe's experiments are likewise open to the same objection. In the experiments which follow, the blind and anosmic animals furnish complete control in tests of this kind.

latch was thrown up from its socket. The usual large experimental cage was placed over the small problem box. The door of this latter box always, *e. g.*, faced the north. The rat to be tested was admitted through the side of the experimental cage facing east; consequently, in order to reach the door of the problem box, he would have to go almost due west. If, after the animal learns the problem in this way, the door of the problem box be made now to face west, and the animal be admitted on the north side of the experimental cage, his reaction times for the first two or three trials are considerably increased.[1]

It would seem then, if these observations are at all worthy of trust, that the rat is in some way sensitive to changes in absolute direction. This position is possibly further supported by the results we obtained from experiments on the rotation of the maze. Our blind (3), anosmic (2), partially deaf (2) and normal (2) rats were again the subjects under observation. These rats were trained to the maze with the entrance south (see cut, p. 10). They have been the subjects of so many experiments that they were thoroughly automatic in their reactions. The rats, in the normal reactions from day to day, maintained a remarkably low mean variation in their time; consequently any disturbance in their reactions which may be noted below is unquestionably due to the effects of the modification of conditions.

The first test was made after the maze had been rotated 180°, *i. e.*, absolute direction of north and south interchanged. (*It must be observed that in all of these rotation experiments no single relation within the maze was altered thereby. The animal still turns to the left or to the right as he has always done—nothing is changed except the absolute direction.*) The normal reaction time of these rats should be placed approximately at .26 min.

Effect of Rotating Maze 180° Upon, (*a*) Anosmic Rats.

Rat I. 1st trial. No errors but slow. Time: .41 min.
Rat II. 1st trial. No confusion. Time: .28 min.

[1] These statements are made on the basis of chance observation. We did not at the time appreciate the possible bearings of the phenomena observed and consequently made no effort to control them.

Second Day's Experience.

Rat I. 1st trial. Badly confused. Ran into *A*. Came back home. Took cue there but ran into *B*. Forward again but ran into *F*. Butts into cross pieces and sides of galleries with her nose. Time: 1.00 min.

2d trial. Ran full length of *F*. Time: .35 min.

3d trial. Hesitated at turns. Time: .28 min.

4th trial. Became confused after starting and returned home. Time: .45 min.

Rat II. 1st trial. Lacks confidence. Makes no errors but hesitates at every turn. Time: .41 min.

2d trial. Repeated above. Time: .31 min.

3d trial. Made error by running into *A*. Started into *B* but withdrew and went on. Time: .38 min.

4th trial. Perfect. Time: .28 min.

(*b*) Partially Deaf Rats.

First Day's Experiences.

Rat I. 1st trial. Badly confused. Made every error in the maze. Time: 1.45 min.

2d trial. Perfect. Time: .26 min.

3d trial. Perfect. Time: .28 min.

Rat II. 1st trial. Ran into cul-de-sacs *A* and *B*. Two other marked hesitancies. Time: .66 min.

2d trial. Made two errors in cul-de-sacs. Time: .46 min.

3d trial. Error in *C*. Time: .55 min.

4th trial. Error in *E*. Time: .45. min.

Their second day's experience with the maze in this position showed practically normal reactions.

(*c*) Normal Rats.

First Day's Experiences.

Rat I. 1st trial. Absolutely lost. Ran into all the cul-de-sacs. Back home, etc. It was like learning the maze for the first time. Time: 2.31 min.

Rat II. 1st trial. Badly confused. Errors in cul-de-sacs and hesitancies. Time: .68 min.

Second Day's Experiences.

Rat I. 1st trial. Ran into *B*. Hesitant. Time: .48 min.

2d trial. Error in *C*. Time: .55 min.

3d trial. Error in *C*. Time: .35 min.

4th trial. Perfect. Time: .25 min.

Rat II. 1st trial. Butted head squarely at first turn but was very rapid in movement. Made no errors. Time: .25 min.

2d trial. Slight hesitancy at one turn. Time: .25 min.

3d trial. Perfect. Time: .21 min.

4th trial. Perfect. Time: .21 min.

(*d*) Blind Rats.

Here we received our second shock. *The blind rats were not in the least disturbed by this change in the position of the maze.* In view of the unchanged

behavior of the blind rats, the first thought which occurred to us, naturally, was, that vision must after all play some rôle in the maze association. But since we had accumulated so much evidence against this view, we decided to reserve judgment until the blind rats, as well as the others, had been given other trials with the maze in different positions. All the rats were run through the maze with the entrance north (its position in the tests described immediately above) until they were thoroughly familiar with it in that position. This took about 12 trials (3 days). The maze was then rotated 90° throwing the entrance to the east.[1] *All the rats were badly confused. The confusion in nearly all cases being more marked than in the preceding test.* We cite in detail the behavior of the blind rats.

Rat I. 1st trial. Started to right instead of to left. Prolonged hesitancies but no pronounced errors of turn. Time: .46 min.
2d trial. Perfect. Time: .21 min.
3d trial. Perfect. Time: .20 min.
4th trial. Perfect. Time: .26 min.

Rat II. 1st trial. Uncertain in all his movements. Went full length of *B*. Bumped into wall. Went back into *A*. Hugged wall all the way. Absolutely lost. Went back to entrance. On return butted squarely into wall at first turn. Ran full length of *F*. Time: 2.41 min.
2d trial. Butted wall. Hesitancies. Full errors in cul-de-sacs. Time: .51 min.
3d trial. Bumped into wall. Started into *B*. Time: .38 min.
4th trial. Bumped into wall. Started into *B*. Time: .40 min.

Rat III. 1st trial. Butted squarely into walls several times. Much hesitation. Time: .35 min.
2d trial. Perfect. Time: .21 min.
3d trial. Perfect. Time: .23 min.
4th trial. Perfect. Time: .23 min.

After all the rats had become accustomed to the maze in this position (entrance east), the maze was turned back to its original position (entrance south): *All the rats were again confused—the blind rats being as much affected as the others.* After the rats had again become familiar with the old position of the maze, it was again rotated 90°, this time, however, the entrance was placed to the west. Marked confusion, similar to that described above, was observed in the behavior of the rats; the blind rats, however, being the least affected by the change.

As a further check upon our work, we ran our rats again with the maze in the original position (entrance south), until their reactions were normal; we then carried the maze as it stood,

[1] *I. e.*, 270° from the original position.

straight south for a distance of about eight feet. This markedly changed the visual relation existing between the maze and that part of the room in which it usually stood. In thus moving the maze, it happened that it rested in its new position midway between two large windows: *None of the rats was affected by this change.* We then altered the direction of the light (strong sunlight) by pulling down first the shade to the left of the maze, and then the one to the right and finally both. Altering the direction of the light and decreasing the intensity of the light—even while the rat was *en route*—had not the slightest effect upon his behavior.

While we dislike to leave this problem in its present state,[1] it is necessary to do so, in view of the fact that its discovery came so late that we had no time to construct apparatus suitable for carrying out further tests upon it, before the publication of this paper was made necessary. We are at present engaged in constructing a self-rotating maze, the position of which can be changed to any desired angle. The tests must be made undoubtedly upon a large number of animals, normal, anosmic and blind, and possibly upon animals whose semicircular canals have been destroyed. And since the blind rats did better in these tests on the whole than those with vision intact, we must construct our maze so that the visual environment of the maze will rotate *pari passu* with the maze itself.

Until our experimentation has gone further, we shall not hazard any 'explanation' of these disturbances in the reactions of the rat. The possibility suggests itself, however, that the semicircular canals are in some way responsible for it, but *how* they are responsible for it—if they do figure in it at all—is at present beyond our knowledge.

[1] Since the above statements were written Miss Vincent has kindly repeated these experiments for the author upon a different maze, the plan of which, however, was identical with the previous one. Four normal animals were used. Great care was taken to keep the maze level both in the position of the original learning and in the rotated position. Her results harmonize throughout with the above. Experiments were also made to determine how sensitive these animals are to changes in position in the horizontal plane. It was found that they are not very sensitive to such changes since one corner of the maze can be lifted as high as 2½ inches without disturbing the reactions of the animal.

PART C.

Summary and Conclusions.

Let us bring together, as briefly as possible, the main facts which the present paper attempts to contribute.

1. We have established the time of the normal process of learning the maze: In our 'normal control series' and in the 'combined,' we have a group of records which, we feel confident, shows all the essential features as regards, (1) the absolute time of the first, second and succeeding trials in the maze; (2) the average mean variation in time of the successive trials; (3) the maximal and minimal consumption of time at any trial; (4) the percentage rate of improvement from trial to trial. Outside of the usefulness of this combined record to serve as a basis of comparison with similar records of other animals, it has the more immediate function of serving as a standard with which the records of the defective rats may be compared.

2. Neglecting the behavior of the two rats whose records have been discussed in the text, we make the assumption with some confidence that vision plays no part in the maze association. In the case of the two rats which proved exceptional in this respect, we believe that we have proven, by the experiments with the miniature lights, that even they were not using vision in any critically discriminative way. In what way the light exerted its function upon their reactions does not appear so clearly—two possibilities are open to us: In view of the fact that they were not so healthy as the others, it is possible to assume that the light exerted a general tonic effect upon their organism; in the second place, we may suppose that the animal really 'sensed' a difference in brightness at the correct turns, and that this became associated with the kinæsthetic sensations of turning (the 'seat' of which possibly may be largely in the eye muscles). This hypothesis would make the

'memory' of the turn still largely kinæsthetic with the added condition that the kinæsthetic series would not function properly at critical places without the assistance of the visual impulse to serve as an 'eye muscle pull.' We ourselves are inclined to accept the first hypothesis, although the fact that the rats really attempted to solve the problem in darkness militates against such a point of view. In all other cases, the rats gave as consistent reactions without vision as with it, viz.: (1) Rats trained to the maze in the light can run it perfectly in the dark; (2) normal rats can learn the maze as readily in the dark as in the light; (3) totally blind rats can learn the maze as readily as normal rats; (4) rats trained to the maze in the light suffer little loss in the accuracy of their adjustments to the maze if deprived of vision.

3. In view of the fact that five of our animals which were deprived of the sense of smell learned the maze in normal time, the evidence seems clear that olfactory sensations have no rôle in the selection of the proper turns in the maze. Added emphasis is given to this point of view, when we recall that two of our anosmic rats learned the maze in 'record breaking' time.

4. Cutaneous sensations cannot, in our opinion, serve as the basis for making the correct turns in the maze, for the following reasons: (1) the vibrissæ, in all probability the most sensitive part of the cutaneous mechanism, can be dispensed with absolutely without disturbing the reactions of the animal, provided sufficient time is given him to 'wear off' the unpleasant 'affective tone' conditioned by their removal; (2) the experiments with the cooled and heated copper plates show that the slight differences in temperature existing in the maze have no influence upon the selection of a given turn; (3) the direction of the air currents in the maze likewise are without effect upon the selection of the turns; (4) after the application of a local anæsthetic to the soles of the feet and to the bare portions of the snout of the rat, his reactions remain unchanged.

5. While none of the animals experimented upon was

totally deaf, still the results obtained from those whose aural sensitivity was markedly decreased show: (1) That audition proper, in all probability, is not contributory to the formation of the maze association; (2) that sensations set up in the tympana by the changing pressure of the air columns (really belonging to the cutaneous group) do not aid the rat in selecting the turns. We are in no position at the present moment to make any statements concerning the function of the static sensations in this association. The rotation experiments suggest, however, if our facts are genuine, either that static sensations have a rôle or else that the rat has some non-human modality of sensation which, whatever it may be, is thrown out of gear temporarily by altering the customary relations to the cardinal points of the compass ('sense orientation'). We have reason to suspect that such a 'sense of orientation' functions in many orders of animals. At present, we have the facilities at hand to make tests upon the homing pigeon (similar to the ones reported above). Something possibly may come in the way of control over this 'sense,' even if further investigation proves that the human organism is not supplied with it.

6. The sense of taste in rats, at least from the affective standpoint, seems to be a crude affair. They will accept and eat food soaked in high percentage solutions of salt and bitter —food which we should reject even in times of a stockyard's scandal, much more under conditions of hunger similar to those obtaining in the rats at the time these tests were made. The rats' sensitivity to sour substances is apparently more acute than to any of the others, a 1 per cent. solution of tartaric acid being almost uniformly rejected. Most human observers, we believe, would agree that a 1 per cent. solution of tartaric acid is far less disagreeable than either a 5 per cent. salt solution or a .1 per cent. quinine solution. It was found that all percentages of cane sugar which would stay in an aqueous solution at the room temperature were eagerly accepted by the rats.

7. Are we not, then, forced to conclude that the white rat makes the correct turns in the maze on the basis of the intra-

organic sensations—the kinæsthetic sensations coupled with the organic probably, and possibly with the static? If we grant that the negative evidence brought forward in this paper is sufficient to establish the fact, that the process of correct turning in the maze is not 'controlled' in the rat by his extra-organic sensations, then it becomes our duty to hazard some hypothesis as to how the 'kinæsthetic series' is controlled. Turning for a moment to the human behavior and attempting to analyze the reactions of one in a situation similar to that described above for the rat, we should find in all probability that the 'kinæsthetic series' of such an one could not be controlled without tactual or visual data.

So far as we know, no human being has ever had to learn so complicated a path under conditions where all the extra-organic sensations were made impossible. It would be instructive to have this maze, as it stands, made large enough for human beings. It could be built as a dark room and with differences in contact values (floors, walls, etc.) approximately equalized. The galleries should also be made large enough to permit the experimentee to move without allowing his body to touch the walls. Suppose now that we impose the following conditions upon our experimentee: (1) That all bare parts of the body be thickly covered and that the arms be bound to the sides; (2) that both ears and nostrils be tightly plugged? Under such conditions, *could he ever learn to run down the center of the galleries for exactly the proper distances, making the correct turns without in any way touching the sides of the galleries or without feeling for the openings, and without making an error or even slowing up his pace* (even provided he were allowed to count his steps)? Granted that he had the ability, could he ever become perfect in this operation, so far as the elimination of errors is concerned, at from 7–10 trials, as many of our rats do? We have serious doubts as to whether the human being could learn the maze under such conditions. Certainly we believe, that if he could so learn it, it would be at the expense of infinite time and infinite patience. And yet, as is well known, we do perform most of our habitual

actions largely by means of our kinæsthetic sensations. Even the traversing of familiar pathways becomes more and more automatic. If some one turns out the light on us before we are out of the room, we can continue our way without serious mishap. Some people can travel all over a large house filled with familiar objects without colliding with any of them. But such automatism while executed largely in kinæsthetic terms is every where *'controlled' by slight contact experiences* (when vision is excluded of course).

In our own case visual *imagery* would play a preponderating rôle. If we come in contact with a chair in a very dark room, the objects in which are familiar to us, the visual image of it is immediately aroused. Along with this image, there follows a series of images of the objects adjacent to the chair to which the natural spatial relations more or less clearly adhere. Visual-motor images of ourselves as turning (impulses) immediately arise which must, as is usually stated, reach the proper degree of intensity (James' anticipatory image) before the ensuing adjustment is made which is to lead to the reëstablishment of the automatic series. No one would dream of affirming that such a complexity in the cortical processes as this would call for could exist in the case of the rats.

Small in his discussion of what goes on in the 'mind of the rat' says that he has found 'unmistakable signs of the presence there of motor images.' It is a little hard for us to see the necessity of motor images or even the functional value of images in a situation like that offered by the maze. Introspection in our own case shows that after we have thoroughly established a series of motor coördinations, we never thereafter are distinctly conscious of the separate elements of the automatic series; and even in the learning process we often establish such a series of coördinations in a more or less trial and error fashion.[1] Why, then, in the case of the rat, need we assume the presence of motor images? The only possible times we could assume that the cortical conditions in the brain of the

[1] That is to say we do at times establish a series of simple coördinations without having distinct imagery of any kind corresponding to the separate steps in the series, even in the learning process.

rat are at all sufficiently complex to warrant the arousal of an image, would be in cases of hesitancies or inhibitions shown at the turns, but even here we need not assume that images necessarily arise.

In such cases, however, as in certain others which we shall mention below, it is necessary to assume that the animal in some way 'senses' that the group of sensations arising at the moment is not 'familiar.' The neural impulses underlying these 'unfamiliar sensations' will sooner or later inevitably discharge into the motor region and there release a motor impulse; if the adjustment then following is 'sensed' as 'familiar,' the animal goes ahead as before; if as 'unfamiliar,' he is at the mercy of his organism until a familiar one is hit upon.

The discussion so far has not touched upon the 'control factors' in such a series of kinæsthetic sensations. To get even a working notion of how the various correct turns in the maze are made, it seems necessary to fall back upon the assumption which Small has already virtually made, viz., *that along with the kinæsthetic series as an integral and indissoluble part thereof, goes the 'sensing' of the amount of effort put forth.* Making this assumption, let us see how it works out practically. In the maze, there are 27 turns from the entrance to the food-box. Let us assume that each straightaway with one turn equals one complete unit of a system of 27 such units. Let us suppose, also, that the act of turning is the 'control' element in each unit. What leads up to the act of turning? The 'feeling' (probably only vaguely 'sensed') which may be expressed anthropomorphically in these terms: "I have gone so far, I ought to be turning about now!" This would call for nothing but a crude sensuous level of intelligence plus the element of 'association'—and neural 'synergy' at that, is all we ask for. This act of turning is the completion of the unit—the touchstone by which the success or failure of the act as a whole is tried. If the turn is made before or after the proper amount of energy is expended, the animal runs into the wall or else goes past the true entrance: *If, however, the turn is made at the proper stage* (and it has been shown that blind rats de-

prived of their vibrissæ can make these turns without allowing their bodies to touch the edges of the openings at the turns), *the animal may be supposed thereby to get a 'reassuring feeling' which is exactly comparable from the standpoint of control to the experience which we get when we touch a familiar object in the dark.*

If this is really a correct statement of the *modus operandi* of the process of traversing the maze, then it seems to us that the matter might be tested experimentally. Up to the present time, however, we have made no carefully controlled tests which might support this view. The reason for this lies again in the unsatisfactory maze with which we have been working. We have planned to construct a maze in which the straightaways can be shortened or lengthened without disturbing any of the *turning* relations. The advantage of this is obvious—if the above statement of the process is true, then for aught we can see to the contrary, at the present moment, a trained blind rat running the maze without vibrissæ ought to attempt to turn at the correct distance regardless of whether the entrance is there or not. (And so far as we have any evidence to the contrary, the normal rat running the maze without vibrissæ would behave in the same way.) Speculation, however, is fruitless and our hypothesis must await further work before it receives positive support.

In conclusion, we may say that the present paper does not attempt to advance our knowledge, on the positive side of the questions involved, one whit beyond the point where Small has already led us; we have attempted rather to convince ourselves by further experimentation that his conclusions to the effect that visual, olfactory and tactual sensations do not furnish the element of control in the maze association are correct. In reworking this field, we have supported everywhere the negative contentions of Small. We no more than he, offer *positive evidence* that the kinæsthetic sensations are the all-important and only necessary factors in the maze association. Both of us alike used the method of elimination. But in leaving even the negative side of the question as Small left it—practically

without convincing support, it became necessary for some one to go over his ground, using more rigid methods of experimentation. We feel that we are now in a position to *begin the study* of the positive aspects of the problems offered by the behavior of the rat in forming the maze association.

APPENDIX.

After our work had been completed up to the point indicated above, it was suggested to us that we had not taken into account a possible source of error in our final conclusions. It may be argued that we can easily conceive of an animal which, in its normal condition, might use its eyes with a good deal of effectiveness in making minute adjustments, yet when deprived of vision might learn very quickly to depend upon the sense of smell. That vision may be substituted for smell in adjustments made hitherto upon the basis of smell alone, when for any reason the latter function is interfered with, is likewise an equally possible contention.

Believing that the proof of the establishment of the maze association in an animal deprived of the possibility of receiving most of the important extra-organic sensory stimulations would add the needed confirmation to our previous work, we removed the eyes, the olfactory bulbs, and the vibrissæ simultaneously from a young male rat on September 6, 1906. There is no need to describe the operations. Naturally recovery was slow in this animal. A certain lack of tonicity was observable. This was due in all probability to the loss of the customary afferent stimulation coming from eye and nose. The animal finally completely recovered and is still alive (March 1, 1907) and in absolutely perfect condition. He shows the same eager curiosity, which is so characteristic of the normal animal.

During the first thirty days of his recovery, the animal showed no signs of hunger. At the end of thirty days, he was tried in the maze, but owing to the lack of the hunger stimulus, he made no progress in learning it. On account of his rather feeble condition, we were afraid to keep him long from food. On being admitted to the maze, he would advance for a few steps and then settle down quietly in one corner. If stimulated, either by sounds, to which he was extremely sensitive, or by slight contact stimulations, he would get up and continue

his journey, but even after reaching the food he would make no effort to eat it.

We then forced the rat to live in the food-box of the maze for several days (shut off, of course, from the rest of the maze). We supposed the animal would make some effort to return home if taken out of the nest at *H* and put down in the entrance at *O*. This was found not to be the case—the animal apparently being better satisfied to loiter in the galleries than to remain at home.

Finally, we adopted the rigorous procedure used with the normal animals. The rat was kept hungry and was fed completely only after finishing his daily quota of trips through the maze. This method worked like a charm. The animal grew physically stronger, his appetite became normal and he began to gain in weight. He began at once to learn the maze and finally became the usual automaton. The elimination of errors went on more slowly, however, than in the case of the normal animals and consequently the number of trials is greater in his case than in the former.

After the animal had thoroughly mastered the maze, his reaction times were not greater than those of the normal animals nor were they more variable than those of the normal rats. The following ten consecutive records illustrate this point admirably:

.19 min.	.22 min.
.23 min.	.23 min.
.21 min.	.23 min.
.16 min.	.20 min.
.21 min.	.23 min.

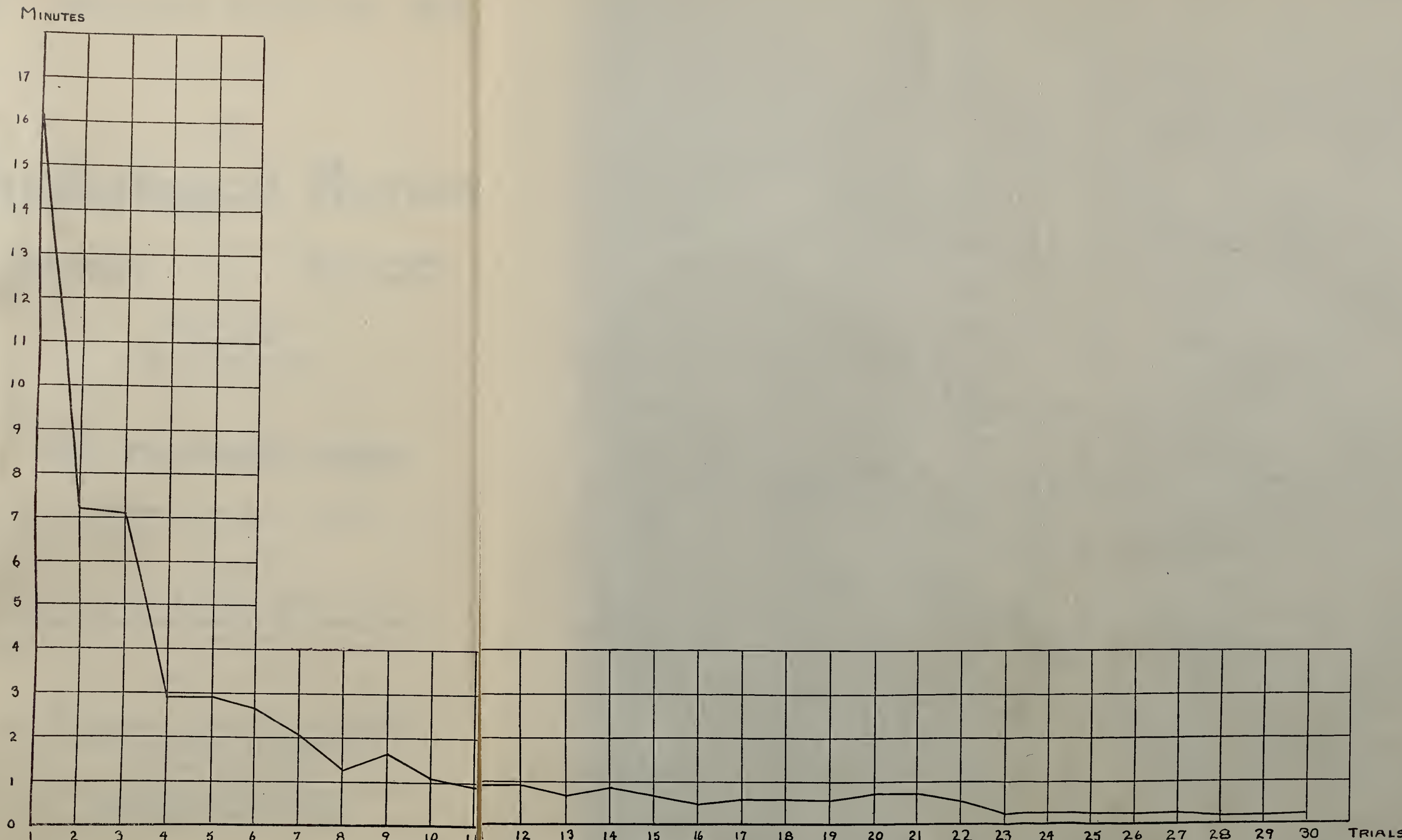
These records were taken immediately after the animal had just learned to eliminate all errors. From this point on he could be depended upon to do his work with steadiness unless some disturbing sound was made while he was passing some important turn in the maze. If even a slight noise were made at such a time, he would make a serious error, and unfortunately, this error was made on the succeeding trips at the same place again and again. If one such error were made, the connectedness of the whole series of movements was likely to be

interfered with—the animal would get hopelessly lost. When in this condition, there was nothing left to do but to put him back in his cage and let him 'sleep it off.'

One rather amusing error was made by this animal which persisted for a long time after all others had been eliminated. He would dash correctly from the entrance at *O* around as far as *r* without making an error. He would invariably dash by *r* the full length of his body (into *B*). He would swing around immediately to his right, dash into *r* and make up for the lost time during the rest of the journey. This mode of procedure at *r* became fixed and we doubt if the error would ever have been eliminated had not a happy accident intervened at precisely the psychological moment. But on one of rat's journeys the operator happened to cough once just as the animal approached *r*. The disturbance caused him to turn to his right, which led him directly into the right path. After this, the animal eliminated this error (having occasional relapses). We mention this incident because, in our opinion, it proves quite conclusively that the rat had no supra-sensitive mechanism for detecting openings at a distance.

The reactions of this animal also were disturbed when the position of the maze was altered with respect to the position of original learning. The tests upon this point were made exhaustive.

Finally, we may say, that in some ways the behavior of this animal was more interesting, with respect to his sensitivity to sounds, and to his adherence to certain errors, than that of any of the other defective animals used in the experiments described in this paper. If time had permitted, we should have used more than the one animal, but we believe that even this one animal establishes our main contention, viz., that the intraorganic sensations are the only necessary sensory factors in forming the maze association.



Curve III. Showing the average time of nineteen normal rats in learning the maze.

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EDITOR'S PREFACE.

This issue of the Yale Psychological Studies completes the first volume of the New Series.

Mr. Charles H. Smith, the Mechanic in the Laboratory, drew a number of the figures used in the papers here published and also constructed various pieces of apparatus used in these investigations.

TABLE OF CONTENTS.

TONAL REACTIONS.

EDWARD HERBERT CAMERON, Ph.D.,

Instructor in Psychology, Yale University.

I. Introductory; Description of methods that have been used in studying tones.

II. The method of this investigation was the tracing upon smoked paper of vibrations from the voice which were transmitted by means of a diaphram to the smoked paper.

III. Preliminary tests of this method.

IV. Accessory apparatus for producing standard tones. Organ pipes were used for these tones.

V. Summary of previous investigations of distractions.

VI. Results of experiments.

Series I. The attempt to sing a uniformly sustained tone is not successful. The pitch varies from moment to moment. The beginning of the tone is markedly irregular and there is a tendency to raise the pitch towards the end of the tone.

Series II. Maintaining a tone uniformly with short intervals of rest gives results similar to those of Series I.

Series III. Imitating standard tones shows different degrees of ability in different individuals and in the same individual for different tones. There is a general tendency to sing higher than the standard.

Series IV. Imitating tones as above with organ pipes sounding as distractions results in departures sometimes in the direction of the distracting tone, sometimes in the opposite direction. There is usually a harmonious relation between the sung tone and the distracting tone.

VII. Discussion of results.

I. Introductory.

The voluntary production of vocal tones involves at once the recognition of a series of sensory impressions and a highly developed reaction to these sensory impressions. Even so simple a process as that of maintaining or repeating the simplest tone calls for a constant control of the vocal organs and a constant exercise of some degree of active attention. The investigation of vocal production of tones is, accordingly, important for two reasons. In the first place such an investigation will throw light on the relation between the type of reaction and sensation involved in articulation, and in the second place a

contribution can be made to the solution of the general problem of the nature of active attention.

Many methods have been used for the study of vocal tones. The following summary of some of the chief methods employed may serve as an introduction to the description of the method used in the present investigation.

Scott's[1] phonautograph consisted of a large trumpet closed at one end by a thin membrane connected with a small recording lever. The vibrations communicated to the air from the voice were transmitted by means of the membrane and lever to the smoked surface of a revolving drum. Various forms of apparatus not differing essentially from Scott's have been since devised and the apparatus used in the present study is a modification of the same principle.

An important modification of the phonautograph was made by Blake,[2] who removed the difficulty due to the inertia of the levers and the friction of the recording point by attaching a small mirror to a telephone plate and photographing the ray of light deflected from a heliostat. The well-known manometric flame has also been photographed for the purpose of investigations in the field of phonetics.

An apparatus of a somewhat similar kind was devised and used by Hensen[3] to demonstrate the constancy in pitch with which a sung tone can be maintained. The flame from a manometric capsule was reflected in a mirror attached to the end of one prong of a vibrating standard fork. A tone sung into the capsule with the same frequency as the fork causes the flame to reflect but one point in the mirror. The octaves 2:1 and 3:1 reflect two and three points respectively. Other ratios make the flames appear twisted together, the number of points varying with the ratio. Thus, the ratio 3:2 has three points and the ratio 4:3 four points. A slight variation in the pitch causes the figures in the mirror to rotate around a vertical

[1] Scott, *Inscription automatique des sons de l'air au moyen d'une oreille artificielle,* 1861.

[2] Blake, The use of the membrana tympani as a phonautograph and logograph, *Archives of Opthal. and Otol.,* 1876, Vol. V., No. 1.

[3] Hensen, 'Ein einfaches Verfahren zur Beobachtung der Tonhöhe eines gesungenen Tons,' *Arch. für Anat. und Physiol.* (Physiol. Abth.), 1879, p. 155.

axis, the rate of rotation depending upon the amount of variation. If the sung tone is lower than the standard tone, the flame appears to move in the direction in which its tips are pointing; if too high, it appears to move in the reverse direction.

Klünder's[1] method for determining the accuracy with which a tone can be reproduced by the voice was to obtain records from the voice and an open organ pipe simultaneously. The record from the pipe and that from the voice were obtained by means of two phonautographs so situated that the two records were made side by side on a smoked paper. The number of vibrations from the organ pipe was then compared directly with the number made by the voice during the same time. By this somewhat rough method of comparison, Klünder found an average error in pitch of from $\frac{4}{10}$ of 1% to $1\frac{1}{2}$% in tones sung under favorable conditions by several persons who were trained musicians.

Among the more recent methods of studying the pitch of tones is Seashore's[2] tonoscope. This apparatus is constructed on the same principle as the stroboscope, the vibrations being made visible on the moving surface of a drum, by the action of intermittent flashes of light from a manometric flame. The drum is covered with white paper, on the surface of which are parallel rows of equidistant dots. The first row has seventy-three dots and each succeeding row contains one more than the last.

The speed of the drum is regulated so that intermittent flashes of light of the same frequency as a standard tone cause one of the lines of dots to appear as if standing still. The sung tone is then projected on the screen by means of intermittent flashes from a manometric flame. The pitch of the tone is indicated as before by the number of dots in the line that appears to stand still. The sung tone can thus be compared with the standard tone.

[1] Klünder, 'Über die Genauigkeit der Stimme,' *Arch. für Anat. und Physiol.* (Physiol. Abth.), 1879, p. 119.

[2] Seashore, 'A voice tonoscope,' *University of Iowa Studies in Psychology*, 1902, Vol. III., p. 18.

While the tonoscope has the advantage of presenting to the subject the visible results of the tone while it is being sung, and also of facility in reading, it has on the other hand the disadvantage of giving only an approximate result. The pitch not being uniform for even very short periods of time, it is necessary to select the predominating pitch.

II. Apparatus.

The recording apparatus used in the present investigation consisted of a round rubber telephone receiver, a vertical cross section of which is represented in Fig. 70. The box

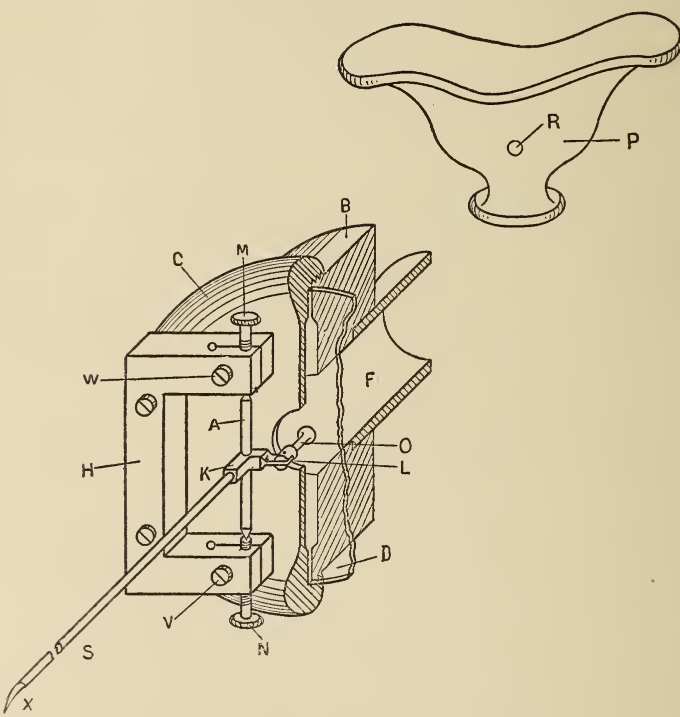

Fig. 70.

(*B*) is provided with a cover (*C*) of the same material which may be screwed tightly to the face of the box. Between the front edge of the box and the cover is a diaphragm (*D*) of thin mica, which is held firmly in position by the cover, when

screwed down. The diaphragm is 5.3 cm. in diameter. Glass diaphragms have also been used, but with less satisfactory results.

The cylindrical chamber (*F*) communicates directly with the air chamber back of the diaphragm. An aluminum mouthpiece (*P*) is attached to the outer edge of (*F*) by a small piece of rubber tubing. In the later experiments a long flexible tube was substituted for this form of connection between (*F*) and the mouthpiece (*P*). A small hole (*R*), 3 mm. in diameter, is bored in the mouthpiece to allow the escape of the air forced into the chamber at the moment the tone is sung.

There is screwed to the front of the box a piece of brass (*H*), shaped as shown in the figure, and used for the purpose of holding the adjustable screws *M* and *N*.

M and *N* are held securely in position by the set screws, *V* and *W*. *M* and *N* are fitted with jewel bearings in which play the tapering ends of the steel axle *A*.

To the axle is attached the aluminum right-angle piece *KL*. *K* carries a straw (*S*) to the end of which is fastened the recording point *X*. This point is made of hammered brass, carefully cut to a point and polished. Such a point is fine enough to make a sharply defined line on smoked paper and the lamp black does not adhere to it.

The other arm *L* is attached by a joint to a smaller link (*O*) of aluminum which passes through an opening in the middle of the box cover, and is fastened to the center of the diaphragm by a drop of glue.

There is thus provided a system of continuous levers from the outer surface of the diaphragm to the recording point, so that movements of the diaphragm caused by the singing of tones into the mouthpiece or by any other means are magnified, and may be recorded on a belt of smoked paper. Since it is desirable to obtain very long series of records a long belt of smoked paper is used. The belt passes between two drums placed fifteen feet apart. It is smoked at one of the drums and after the record is made is shellacked from behind.

A portion of a typical record obtained in this way is shown in Fig. 71. The upper line (*A*) of the figure is the time line

obtained from a marker connected with a Kronecker interrupter, set to mark periods of 100 sigmas. The lower line consists of vibrations from the voice transmitted through the diaphragm.

Fig. 72, page 233, shows the apparatus used for reading the record. It consists of a cast iron base (*A*), 41 cm. long by 16 cm. wide, fitted with a T-shaped slot, into which fits the T-shaped piece, *B*, of the same material. The top surface of *B* is 9.5 cm. wide, its width being a little greater than that of the glazed paper used for the records.

The record (*C*) is placed upon (*B*) and held in position

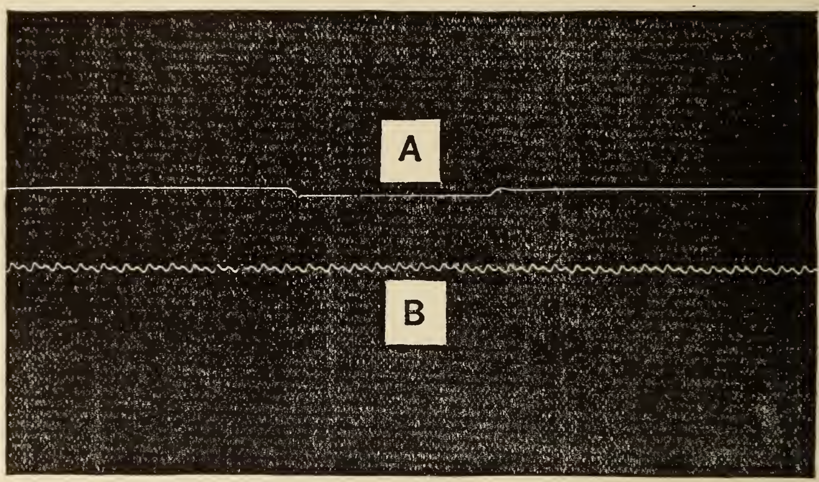

FIG. 71.

by the brass strips, *D* and *E*, which are in turn held in position by the thumb screws, *H, H, H, H*. The inner edges of these brass strips are notched, the tooth-like projections being very accurately spaced, and so placed that the line drawn between any one of the points on the one side to the corresponding point on the other side is exactly parallel to the line of direction in which B moves as it slides in the grooves. The brass gib, whose end only is represented by the dotted line *J*, prevents any loose motion in *B* and still allows perfect freedom of sliding in the base *A*.

M and *N* represent pieces of transparent celluloid, one and

one-half millimeters in thickness, their under surfaces being parallel to that of *B* and at such a level as to just allow for the thickness of the record paper between *B* and the celluloid pieces. On the under surface of *M* and *N* near their inner edges are drawn fine hair lines, *S* and *T*, perpendicular to the line of motion of both *B* and the celluloid plates. The lines were scratched into the surface of the celluloid with a sharp-pointed knife. The inner edges of the celluloid pieces are bevelled to prevent refraction. These celluloid pieces are screwed to T-shaped carriages of bronze which slide in a groove in the metal base, their line of motion being parallel to that of *B*. The brass nuts *P* and *R* are also attached to these strips and the movement of *M* and *N* in either direction is accomplished by the turning of the screws *X* and *Y*.

The screws *X* and *Y* are accurately turned, the distance between the threads being two millimeters. *V*, the head of the screw *Y*, is graduated into forty equal divisions and numbered as shown in the figure.

The record (*C*) is laid upon the surface of *B* and the brass strips *D* and *E* placed over it. The time line *I* is brought exactly underneath one of the tooth-like projections at one end and a corresponding portion of the time line *F* is placed under the corresponding tooth on the other side.

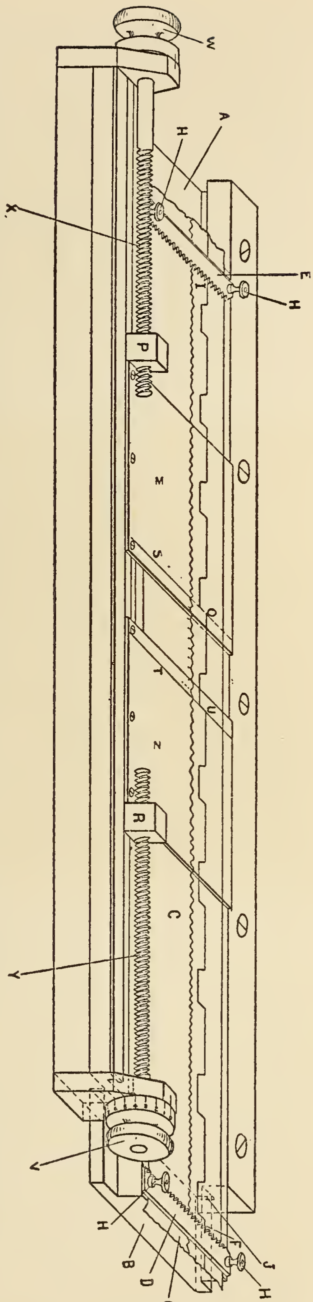

FIG. 72.

The projections on the brass strips being numbered, this is easily accomplished. When this has been done, the brass strips are screwed down upon the record, which is thus kept in such a position that the time line is parallel to the line of motion, and is thus perpendicular to the hair lines *S* and *T*.

By turning the screws (*V*) and (*W*) the hair lines (*S*) and (*T*) may be brought so as to cut corresponding points, such as (*O*) and (*U*), in two successive vibrations of the time line. Suppose the hair line at the left cuts one of the tone vibrations in the depression of a wave, as shown in the figure. The number of whole vibrations occurring during a tenth of a second may then be easily counted. In order to determine the fraction of a vibration left over, the screw (*V*) must now be turned so as to move the hair line (*T*) till it cuts the last whole vibration at a point corresponding exactly to that at which (*S*) cuts the first vibration of the period which is being counted. The number of graduations through which (*V*) has turned having been noted, the screw should be turned until the space of a full vibration has been traversed by the hair line *T*, and the corresponding number of graduations noted. The ratio of the former determination to the latter is the fractional vibration which it was required to measure. By sliding *B* to the left or right this process may be repeated so that a length of about 30 cm. may be measured without making a readjustment of the record.

III. Preliminary Tests of the Recording Apparatus.

Before beginning the experiments on vocal tones, a series of preliminary tests was made with physically produced tones for the purpose of determining the accuracy with which the diaphragm responded to air-vibrations, and recorded small differences in pitch. For this purpose a 100 V.D. electrically driven tuning-fork was used. To the end of one prong of this fork was attached a small mica disc. The fork was set in vibration and held so that the disc was in front of the mouth-piece of the diaphragm. The same 100 V.D. fork was connected with a marker which recorded its vibrations upon the smoked paper belt parallel with the diaphragm lever. At the

same time another marker recorded tenths of seconds from the Kronecker interrupter. There was thus obtained side by side a record of the vibrations of the fork as transmitted by the diaphragm, and one directly through a marker from the same fork. By means of the time line from the Kronecker interrupter these two records could be compared with absolute time as well as with each other. It was found in all instances that the frequency of the vibrations of the two records from the 100 V.D. fork corresponded exactly, thus showing that the diaphragm did not modify the rate of the air-vibrations. No account was taken in any of the experiments of the differences in the form of the vibrations. Since these differences are also neglected throughout the investigation it is not necessary for us to enter into a discussion of their character here.

By weighting the prongs of the fork, the sensitiveness of the diaphragm in responding to small differences in the rate of vibration of the impinging waves was tested. The weights were moved backward and forward upon the prongs of the fork, the changes thus made causing small differences in the rate of vibration of the fork. All these small differences were found to be transmitted to the record. Differences of less than one vibration per second were thus recorded. All these tests were repeated, using a 250 V.D. fork and with similar results.

Comparison of the different parts of the record thus obtained from a fork was also made, with the result that in each case the record showed the uniformity of the pitch of the tone at different points throughout its length. This test was also made with organ pipes and special attention given to the beginning of the tone which will be shown later to be a variable part of sung tones. In no case was there found to be any difference between the frequency of vibration at the beginning of the record and at other points throughout its length. We are, therefore, fully justified in concluding that the inertia of the recording point due to friction was a negligible quantity.

IV. Accessory Apparatus.

A series of physical tones was necessary for various parts of the experiment. These were obtained by using a series of

labial organ pipes made of metal and wood. The metal pipes were taken from the open diapason stop of an organ and were selected because of the comparative ease with which they could be imitated by the human voice. The number of pipes was twenty-five, comprising the two octaves D# (302.8 vib. per sec.) to D# (75.5 vib. per sec.), including half tones. Sixteen higher pipes ranging from D# downward to C were of zinc and were open pipes. The remaining nine, comprising the lower notes from B to D# were closed wooden pipes. The qualities of the two kinds of pipes were not markedly different, the combination being similar to that which is often used in the same stop of small pipe organs.

The air supply for blowing the pipes was obtained from a tank situated in the basement of the laboratory, and was conducted to the research room by means of iron pipes. The tank was supplied with a high pressure of air by means of an air pump, which was operated by the engine of the work-shop.

As the pressure of the air thus supplied was very great and inconstant (10 to 60 lbs. per square foot), an arrangement was necessary to give a steady supply of air at moderate pressure. This was obtained by means of a reservoir provided with a valve which permits a regulation of the air supply. The reservoir consists of two galvanized iron tanks (*A* and *B*, Fig. 73). The smaller tank is placed, bottom upwards, within the larger tank. The wheels *C* keep the upper tank in a vertical position and prevent friction between the sides of the tanks.

Two iron pipes pass through the sides of the larger tank near the bottom and turning upward end almost at a level with the top of the tank. One of these pipes *E* is connected with the air chamber, which communicates directly with the organ pipes; the other *D* is connected with the air supply through the valve *H*.[1]

This valve is opened and closed by the lever *K*, air being allowed to enter when *K* is depressed and being cut off when

[1] During the experiments here recorded, this valve (*H*) was opened and closed by hand. The automatic regulating device shown in the figure has since been added.

K is raised. To the end of *K* is attached the cord *L*. The cord passes over the pulleys *M* and *N*, which are attached to a horizontal beam above the tanks, and is fastened at its other

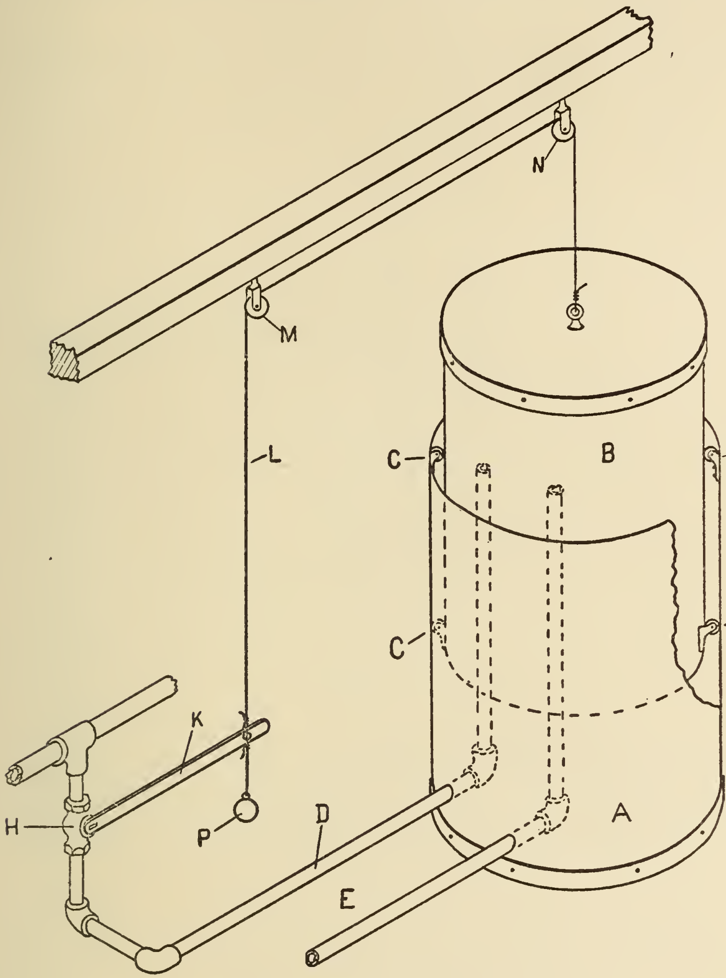

FIG. 73.

end to the upper tank. The small weight *P* suffices to pull the lever *K* down when the tension on the cord is relaxed.

The lower tank is filled with water to a point which is above the level of the uppermost position of the mouth of the upper tank. Air is now admitted from the supply into the

tank *B*. It gradually rises like an ordinary gas tank until the valve *H* is automatically closed.

The air may be drawn off through the pipe *E* for any purpose desired, and the reservoir is automatically refilled by the opening of *K*. There is thus furnished a continuous stream of air under constant pressure for use in sounding the organ pipes.

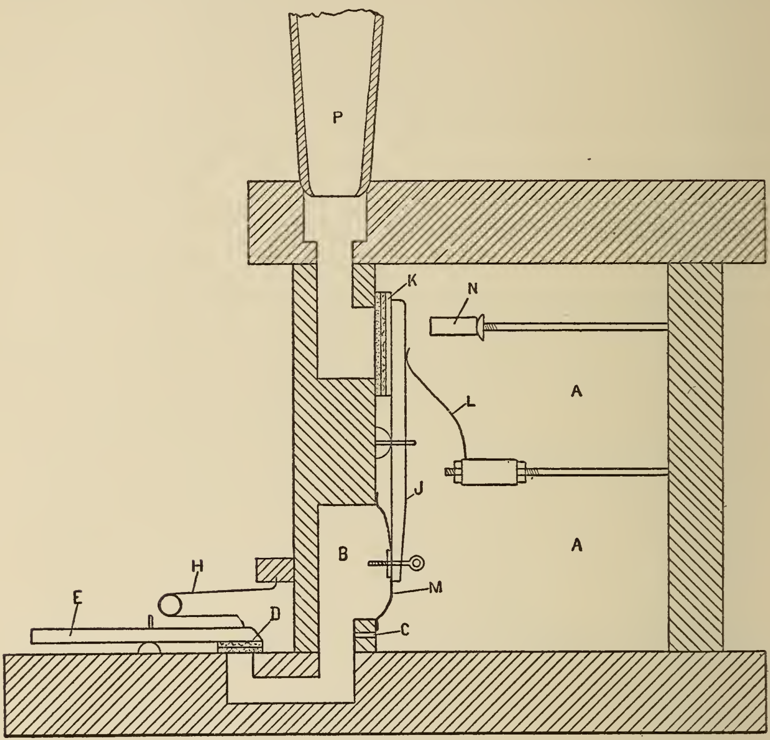

FIG. 74.

By varying the weights used on the tank *B*, the pressure of the air used may be changed at will. During the course of this investigation, the air in the tanks was kept at a fixed pressure of about 15 pounds to the square foot.

The organ pipes were sounded by means of a series of keys and an air chest supplied to the laboratory by Hall & Co., organ builders. Fig. 74 represents a cross section of the air chamber, communicating directly with the organ pipes, and supplied with air through pipe *E* (Fig. 73). The

air is introduced directly into the main air chamber *A*, and part of it escapes through the small hole *C* into the smaller chamber *B*. The latter may be made to communicate with the outer air if the valve *D* is opened. This may be done by pressing the key *E*. Unless the key is pressed at *E* the spring *H* keeps the valve *D* closed. The valve is made of a layer of sheepskin and a layer of felt and the pressure of the spring is thus sufficient to effectually close the opening to the passage of the air.

A similar key *J* controls the passage of the air to the pipe *P*, through the valve *K*. In order to open *K*, the tension of *L* must be overcome and the lower end of the key *J* must move into the chamber *B*, carrying with it the flexible sheepskin partition *M*. If now the valve *D* is closed, the valve *K* is also kept closed by the spring *L* and also by the fact that the pressure in the chamber *A* is greater than atmospheric pressure in the pipe *P*. If, however, *D* is opened, the pressure of the air in *B* is much reduced, since the air escapes much more rapidly through *D* than it can pass through the small hole *C*. As the pressure in *A* remains practically the same, the flexible partition *M* attached to the lower end of *J* is pressed into chamber *B*, overcoming the pressure of *L* and opening the valve *K*. The adjustable post *N* keeps the key from going back too far when released, and thus prevents excessive strain on the spring which is also adjustable.

A simple pressure of the key *E*, therefore, serves to release the air from *A* and sound the pipe. The whole contrivance provides a convenient and simple means for this purpose, and makes use of the air pressure to obtain a quick and noiseless opening of the valve leading to the organ pipe. Each pipe is provided with a separate set of valves of the type described, all opening, however, into the same chamber *A*.

Before beginning the experiments the pipes were carefully tuned. The pitch of each pipe was then determined by recording its vibrations through the diaphragm described above, and measuring their frequency by means of the time line from the Kronecker interrupter. In order to avoid changes in pitch due to variations in temperature, the air of the room

was regulated to 70° F., and in subsequent experiments care was always taken to have the air of the room at this temperature. In making the tests, changes in temperature in the pipes due to handling were avoided by holding them in position by means of cords. Some of the pipes were tested later and no difference in pitch was detected.

The results of the test of the pitch of the pipes were as follows:

Vibrations per Second.

D♯	D	C♯	C	B	A♯	A	G♯	G	F♯	F	E
302.8	286.4	271.8	255.4	240	225	214	200	187	178.1	170	160
152.4	144.2	135.6	126.6	120	112.1	107.1	100	93.7	88.5	85	80
75.5											

V. Summary of Previous Investigations of Distraction.

With the apparatus described it was possible to study the vocal reaction under a variety of conditions. First, in order to lay the foundation for the treatment of all the later questions, an investigation was made of the type of reaction involved in holding a simple tone. Second, the reactor was required to imitate a tone which was sounded in his ear by the organ pipes. Third, the subject imitated a tone in this way, and at the same time another tone from the organ pipes was sounded in order to test the effect of such a distraction.

Before reporting the results of these investigations, it will be advantageous to summarize briefly certain of the investigations of attention which indicate the wide variations produced in different cases by distractions. The methods of measuring the effects of distraction are various, some investigators using reactions and others using comparison of sensations. The general result is, however, that distractions may produce deviations from the normal in every possible direction. This will come out with all clearness in the following summaries.

In connection with his work on reaction-time and attention Bliss[1] made a number of experiments which showed the influence of disturbances of the attention upon the voluntary control of arm muscles. This was done by making a graphic record

[1] Bliss, 'Investigations in reaction-time and attention,' *Studies from the Yale Psychological Laboratory,* Vol. I., p. 1.

of the accuracy with which a person can point steadily to a given spot. The irregular shape of the curve showed that there was a constant movement of the finger above and below the spot. When a strong distracting stimulus was given the control of the muscles was still more uncertain. If the disturbance was slight, however, the added stimulus seemed to render the action steadier rather than otherwise.

In Bliss's reaction experiments no difference in time was detected between reactions in silence and those in which the subject was listening to a continuous sound from a tuning-fork of 250 vibrations per second. When the intermittent beats of a metronome were used as a means of distraction, however, the reaction time was lengthened.

Slattery[1] made a study of the relation of the reaction time to variations in intensity and pitch of the stimulus. The reaction time remained practically constant for variations in intensity, but a decrease in the time of reaction was noted corresponding to the rise of the pitch of the tones used as stimulus.

Swift[2] reported experiments with respect to simple reactions and choice reactions with and without distractions. Distractions were found to lengthen the time of the reactions in both cases. When the distractions were made up of stimuli of the same kind as those to which the reaction took place, there was more disturbance than when the distraction and stimulus were from different senses.

In the Psychological Review,[3] 1894, Münsterberg has an article on the intensifying effect of attention in which he reports the results of a series of experiments carried out in the Harvard Laboratory. The object of the experiments was to find out whether it is true, as usually held, that "when the attention is directed to objects of sense, its effect is not only to increase the clearness and liveliness of the impressions, and strengthen the resulting associations in consciousness, but also to intensify the impressions themselves." The experiments

[1] Slattery, 'On the relation of reaction-time to variations in intensity and pitch,' *Studies from the Yale Psychological Laboratory*, Vol. I., p. 71.

[2] Swift, 'Disturbance of the attention during simple mental processes,' *Amer. J. of Psych.*, Vol. I., 1892, p. 1.

[3] Psychological Review, Vol. I., pp. 39–44.

were arranged in such a way that the intensities of the two impressions of moderate strength could be compared, and at the same time the attention directed toward one and away from the other. The distraction was effected by directing the subject to add numbers before and during the time in which the stimulus was given. In this way four series, as follows, were arranged for each group of stimuli studied: (1) Attention was directed without distraction to each of the two stimuli; (2) attention was directed to the first stimulus but diverted from the second; (3) attention was directed to the second stimulus but not to the first; and (4) attention was distracted when both stimuli were given. These experiments gave the unexpected result that all stimuli appear relatively less when the attention from the outset is directed to them. Münsterberg explains his results by attributing the effect to muscular tensions accompanying the process of attention. As in the well-known weight illusions, "if the sensations of tension be relatively strengthened by expectant attention, the stimulus will appear weaker than if the stimulus itself were to arouse reflexly all the corresponding muscular tensions."

Another series of experiments was made by Hamlin[1] along the same line as Münsterberg's for the purpose of verifying or disproving the earlier results. These experiments proved to be chiefly valuable in pointing out the shortcomings of the method used for distracting the attention. Out of fifteen cases only two showed the maximum of correct judgments in the series without distractions. In six cases the greatest accuracy was reached when the distraction occurred during the time of the first stimulus, and in four cases with distraction throughout. From the introspective evidence these results were interpreted as being due to the fact that the distraction acted as a spur rather than a check to the process of attention. This was possible because addition is not a continuous means of distraction. It was further shown that the duration of the attention is not so significant for accuracy of judgment as the degree of attention. While the distraction brought about by the adding proc-

[1] Hamlin, 'Attention and distraction,' *Amer. J. of Psych.*, Vol. VIII., 1896, p. 3.

ess shortened the duration of the attention, it also afforded, on account of its lack of continuity, brief periods of attention accompanied by a heightened degree of interest.

Following these experiments of Hamlin's a series of tests was made in the Cornell Laboratory dealing with the relative value of different stimuli for the purpose of distractions. The aim was "to discover a reliable measure of the attention by means of some form of distraction which should at least possess the qualities of (1) capability of gradation; (2) continuity; and (3) possibility of general use with normal subjects."

The first of the series of studies is reported by F. E. Moyer.[1] It is concerned with the value of various forms of distractions in connection with the discrimination of various shades of gray, and of the relative intensity of two sounds produced by the falling of ivory balls upon ebony plates. The conclusions reached were that addition, writing words of a sentence in a reverse order, translating into a foreign language, and similar forms of distraction cannot be relied upon to produce the required result. They do not affect all persons alike, nor even the same person at different times and often have no disturbing effect whatever. Of the means tried, odors gave the best results. In all cases the effect of distraction was much heightened when interest in the experiment aroused a strong affective tone in the subject.

L. G. Birch[2] continued the work begun by Moyer and made a more thorough investigation into the value of odors as means of distraction. Fifty scents used to distract the attention in connection with the discrimination of sound intensities of two stimuli decreased the number of right judgments from fifty to no per cent., and in some cases caused an actual increase of the number of right determinations. A table of odors was arranged in the order of their distracting power. It was inferred from the results that distraction in connection with odors may arise in four ways: "by familiar scents that cannot be named; by very familiar, and therefore suggestive, odors

[1] Moyer, 'A study of certain methods of distracting the attention,' *Amer. J. of Psych.*, Vol. VIII., 1897, p. 405.

[2] Birch, 'Distraction by odors,' *Amer. J. of Psych.*, Vol. IX, 1897, p. 45.

(attention on the odor); by unfamiliar, and therefore puzzling, scents; and by easily recognizable scents, whose recognition suggests that the whole experiment is over." Familar scents and uncertainly familiar scents are the least distracting.

Darlington and Talbot[1] carried out experiments to determine the relation between the pitch of a musical note and its distracting power, and in general the value of a musical phrase as a distraction. Music proved to facilitate the attention in connection with the relative estimation of lifted weights, both when played throughout the experiment, and when played in one half only. Very little, if any, connection was discovered between pitch and distracting power.

F. Angell[2] used a number of the above mentioned means of distraction in his experiments on the discrimination of clangs for different intervals. In addition, he used the methods of reading, and listening to interesting literature, and discriminating clangs differing by a small interval. These latter means of distraction were found to be more absorbing than uninteresting and partially mechanical processes like addition and counting metronome beats. Taking into consideration all forms of distraction the accuracy of judgment was increased in two-thirds as many cases as it was decreased, and in a few instances the distractions had no effect. Further, there was little indication that the absorbing character of the distraction, as determined by the subjective attitude of the person observed, was accompanied by a corresponding decrease in accuracy of judgment. On the contrary, it was when the less absorbing forms of distraction were used that the least number of right judgments was recorded.

From the results of these and the preceding investigations, it will be seen that the use of a stimulus to distract the attention may have any of three different results. It may, in the first place, be apparently completely ignored; or, in the second place, it may reinforce the attention; or, lastly, it may divert the attention to the distraction. Distraction is, accordingly, not

[1] Darlington and Talbot, 'A study of certain methods of distracting the attention,' *Amer. J. of Psych.*, Vol. IX., 1898, p. 332.

[2] Angell, F., 'Discrimination of clangs for different intervals of time,' Pt. II., *Amer. J. of Psych.*, Vol. XII., 1900, p. 58.

to be treated as in every case a negative factor. Indeed, it will be shown by the following results that the most productive conception of distraction is one which treats it as an added factor in a general system of conscious processes. As such an added factor it will operate to destroy the earlier state and will lead to a reconstruction of the whole process. The outcome of this reconstruction when compared with the original state may be either positive or negative or in some cases equal.

VI. Experiments.

Series I. Maintaining Sung Tones.

The subjects who were tested in the following experiments were Messrs. Cockayne, Cowling, Ferris, Freeman, Gifford, Porter and Judd. All were familiar with laboratory methods in psychology and practiced in introspection. Hereafter the names of the subjects will be abbreviated in this paper to Ce., Cg., Fs., Fn., G., P. and J., respectively.

It may be of importance to indicate in a general way the natural musical ability and training of each of these subjects, since the singing of a tone is dependent to so great a degree upon these qualities.

Subj. J. has had no musical training and never attempts to sing except in unison with others.

G. and Cg. have also had no training in music. They, however, have no difficulty in carrying tunes and frequently take part in singing when it is carried on in unison with others.

Fn. and Ce. have had no extensive training, but have more than ordinarily good voices, and read simple music.

P. and Fs. have had special training in singing. The latter also plays the pipe organ.

In the first series of experiments the purpose was to ascertain the ability of the subject to maintain the pitch of a tone uniformly. No attention was paid in any of the experiments to other features of the record than the pitch of the tone sung. Differences in the height and form of vibration were neglected.

Each subject was asked to sing three tones and continue singing them for a moderately long period of time. The

directions given with regard to the pitch of the tone were as follows: The subject was asked to sing any tone of medium pitch, a second of low pitch, and a third of high pitch, and to sustain the pitch selected in each case as uniformly as possible throughout the singing. In point of fact the pitch varied with the different subjects approximately two octaves (from 100 to 300 vibrations per second), and the duration of the singing from 8 to 16 seconds.

Tables I. to VI. present the results of these experiments. Each number in the first column of the tables gives the pitch of the tone for a period of 100 sigmas expressed in terms which show the number of vibrations that would have resulted had the same pitch been maintained for a full second. This means that the number of vibrations actually occurring during that period of time has been multiplied by ten in order to express the pitch in the conventional manner of the number of vibrations per second.

The second and third columns give the averages and the mean variations respectively, calculated in groups of each succeeding ten determinations. A short horizontal line drawn between the figures indicates that they do not represent continuous periods of time. The highest and lowest points in each record are starred in the tables. Some of the typical results are given in the form of curves, Fig. 75, p. 256, in which the abscissæ represent periods of 100 sigmas and the ordinates the frequency of the vibrations. The dotted lines represent the portions of the record which were not read. The average pitch is shown by the continuous horizontal line and the highest point which each tone reached is indicated in addition to the initial tone. Curves *A, B, C* represent Fs.'s tones; *D, E, F* represent Ce.'s tones; *G, H, I* represent Fn.'s tones; and *J, K, L* represent P.'s tones.

TABLE I.

SUBJECT J.

High Tone.

	Av.	M. V.		Av.	M. V.
220*			241.8		
230			241.8		
230			243.6		
235.6			243		
236.2			244	241	1.77
230					
240			243.5		
240			245		
234.2			240		
240	233.6	4.78	240		
			244.4		
235			242.5		
237.7			243.3		
240			245.8		
232.5			245		
238.8			244.4	243.3	1.57
240					
234.4			244.4		
236.8			245		
233.5			244		
233	235.8	2.44	246		
			246		
233			245		
233			247		
230			246.2		
237			246		
236.6			245.5	245.5	.73
237.7					
240			242.2		
238.8			247.5		
240			243.3		
237.7	236.5	2.58	250*		
			247.5		
240			246.2		
237.5			245		
240			243.3		
240			245.5		
234.4			243.7	245.4	1.92
240					
240			245		
240			245		
241			245		
240	239.2	1.39	245		
			246		
240			244		
237.8			247		
240			244		
238.2			246.6		
240			244.4	245.2	.80

Medium Tone.

	Av.	M. V.		Av.	M. V.
130			141		
140			140		
140			142.7		
142.5			141.5		
140			142.9	142.2	.97
142.9					
142.9			142.5		
142.7			143.6		
141			143.1		
140	140.1	2.21	142.6		
			143.8		
143.3			142.2		
140			142.6		
140			142.2		
142.1			140		
140			142.7	142.5	.65
140					
141			142.7		
140			146.6		
141			142.9		
140	140.7	0.9	143.8		
			142.7		
141			143.3		
142.1			142.1		
141			143.1		
141.5			143.5		
141.5			142.8	142.8	.47
143.5					
142.7			141.5		
142.8			143.5		
143			142		
141.5	142	.76	143		
			142.6		
141			143.6		
141.5			145		
139.4			143.3		
142.1			142.7		
141.1			143.3	143.3	.69
143.1					
142.1			143.3		
141.5			143.5		
142.1			144		
141.3	141.5	.67	145		
			144.2		
140			144.5		
142.5			144.5		
142			144.5		
141			143.5		
142.6			143.5	144	.69
141					
142.7			142.5		
141.7			143.6		
142.7			144.1		
142.3	141.8	.75	142.2		
			142.7		
142.9			144.2		
145.2			143.6		
142.5			142.9		
142.3			143.8		
141.7			142.5	143.2	.65

	Av.	M. V.		Av.	M. V.
144.2			143.5		
145.2			144.6		
147			145	144.3	0.6
145					
145.5					
145.5*					
143.6					
143.6					
143.3					
144.6	144.7	.89			

Low Tone.

	Av.	M. V.		Av.	M. V.
105*			115.5		
107.3			115.2		
112.5			115.7		
110			116		
111			115		
110			114.2		
113.5			116.3		
113			116.1	115.4	.67
113					
117.3	111.3	2.6	114.7		
			115.9		
112.6			115.2		
113.1			115.2		
113.3			115.9		
110			115.8		
112.2			115.9		
113			116.6		
114			116.3		
112.8			115.2	115.6	.48
112.7					
111.3	112.5	.83	115.5		
			115.6		
110			114.7		
112			115.3		
112.4			114.7		
110.7			115.3		
112.5			116.6		
112.6			117.8		
112.9			116.6		
114.2			115.3	115.7	.76
113.2					
113.7	112.4	.92	115		
			116		
114.8			116		
113.4			116		
114.5			115.5		
114.8			115.6		
115.8			115.2		
113.5			114		
114.4			116		
114.4			116	115.5	.49
114.7					
114.8	114.5	.57	114		
			114.3		
114.6			115.2		
114.4			116.8		
114.8			117.1		

	Av.	M. V.		Av.	M. V.
115.2			118		
114.1			118		
114.3			116.4		
115.9			116.6		
116.1			117.7	116.4	1.15
116.3					
115	115.	.64	116.1		
			118.2		
116			116.1		
116.6			119		
114.8			119*		
115.9			116	117.3	1.26
115.4					
115.6					
114.6					
116.6					
115.6					
116.6	115.7	.57			

TABLE II.

SUBJECT. FN.

	High Tone. Av.	M. V.		*Medium Tone.* Av.	M. V.
230			140*		
230.5			150		
230			146.6		
229.5*			148.4		
230			146.3		
230			146.9		
230.5			148.3		
230			146.6		
230			148.3		
232	230.1	.33	150	147.6	1.86
234			147.3		
232.6			147.8		
235.3			145		
234.7			148.3		
235.5			147.5		
230			148.3		
235.3			147.5		
233.6			148		
234			148.8		
235.3	234	1.19	147.5	148	.68
235.5			148.3		
233.8			145		
233.3			147.3		
234.1			146.2		
238.1*			150		
233.3			147.5		
233.8			147.3		
236.1			145		
234.6			148		
234.6	234.7	1.12	150*	147.4	1.30

Low Tone.

	Av.	M. V.		Av.	M. V.
112.2*			116.8		
120			116.6		
118			117.2		
116.3			116.6		
113.8			115.8	116.2	1.08
112.5			116.8		
115.5			117.5		
114.5			117.5		
118.3			118.5		
117.6	114.8	2.31	118.8		
116.6			116		
120			119		
118.5			117		
116.3			120*		
117.6			119	117.6	1.05

TABLE III.

Subject Ce.

	High Tone. Av.	M. V.		*Medium Tone.* Av.	M. V.
192.7*			115		
206			120		
210			120		
207.9			118.7		
210			114.2*		
207.1			115.8		
210			120		
207.8			118.7		
212.3*			117.8		
207.3	207.1	2.11	118.7	118.1	1.71
210			120		
207.3			117.5		
210			118.1		
210			119.1		
207.6			120		
210			118.5		
208.5			119.5		
207.3			118		
210			118		
208.5	208.9	1.08	120	118.8	.85
210			120		
210			118.5		
208.2			120		
210			120		
211.5			119.5		
210			120		
210			120		
210			120		
210			118.1		
211	210	.43	118.1	119.4	.72
210			118.2		
208.3			120.5		
211.5			122.5		

	Av.	M. V.		Av.	M. V.
208.5			121.4		
210			120		
208.5			118.1		
211.5			120		
210			117.9		
210			120		
210	209.8	.95	118.1	119.6	1.29

Low Tone.

	Av.	M. V.		Av.	M. V.
103.9			101		
100			100		
100.9			103.8		
100.9			98.9		
100			101.6		
100			103.8		
101.5			100		
100			100		
101.5	101.2	1.11	101.6		
			102.2	101.2	1.31
101.4			102.9		
99			100		
100.9			101.3		
101.3			101.4		
100			100.3*		
100			101.8		
100.8			100		
100			102.2		
100	100.1	.56	103.3*		
			100	101.1	1.24

TABLE IV.

SUBJECT CG.

High Tone.

	Av.	M. V.		Av.	M. V.
282.7			284.5		
280			285		
284.3			283.3		
280			283.3		
285.7			284.1		
280			284.1		
285.7			283.6		
284.3			284.1		
282.5			283.5		
285	283.0	1.98	284.4	283.5	.47
286.6			283		
283.3			284		
287*			284		
285			283		
285			285		
284			284		
286			283.9		
288.2			284.2		
285			286		
285.4	285.5	1.11	282.2	283.9	.83

	Av.	M. V.		Av.	M. V.
285			283		
284			282		
285			283		
283.6			282		
285.5			283		
284.5			282		
283.6			282.5		
285			281.1		
283.2			285.5		
286.6	284.6	.87	283.7	282.7	.86

Medium Tone.

	Av.	M. V.		Av.	M. V.
227.5*			243.3		
230			241.8		
228.5			244.1		
232.6			242		
240			243		
240			241		
240			243		
240			241		
240			244		
241.1	235.9	5.07	242	242.5	.86
240			241.8		
242.3			243		
240			241.8		
243.8			242.7		
242			243		
242			243		
242.1			242.7		
242.1			242		
242.7			242		
242	241.9	.78	244	242.6	.56
240			244.5		
242.6			244		
242.1			243		
242.8			243		
240			244		
242.4			243		
242			245		
240			245		
242.3			243		
241.8	241.6	.96	245*	243.9	.76
241.6					
243.1					
242.3					
240					
242.5					
243					
243.3					
243.3					
242.3					
241.5	242.3	.75			

Low Tone.

	Av.	M. V.		Av.	M. V.
121*			131		
123.3			130		
125			131		
125			131.7		
125.8			130		
130			130		
123.1			130		
125.3			128.8		
130			130		
126.4	125.4	2.01	130	130.2	.57
125.6			128.8		
123.6			130		
125.5			130		
127.5			131.2		
127.6			130		
125.8			130		
128.7			128.8		
125.8			130		
128.3			130		
125.7	126.4	1.29	128.7	129.7	.61
130			130		
130			135.5*		
131.6			134		
130			130		
128.8			132		
130			130		
130			128.1		
128.1			128.1		
128.3			127		
132.2	129.9	.9	131.5	130.6	2.1

TABLE V.

SUBJECT P.

	High Tone. Av.	M. V.		*Low Tone.* Av.	M. V.
262.7			125.5*		
262.8			129.8		
265			131.5		
261.6			135		
260*	264.4	1.6	130.9	130.5	2.5
260			133.6		
269.9			131.6		
264.5			136.5		
262.5			131		
264.7	264.3	2.4	135.1	133.3	1.8
265.5			132.4		
270			135		
270			135.2		
262.1			130		
265	266.5	2.7	133.6	133.2	1.6
262.6			132		
267.2			135.5		
266.5			131.3		
270*			133.8		
262.5	265.7	2.5	135.8*	133.6	1.6

Medium Tone.

	Av.	M. V.		Av.	M. V.
202.5*			225		
210			223.1		
222.5			227.5		
223.7			225		
222.5	216.2	8.0	227.5	225.6	1.5
225			224.6		
222			228.6		
225			225*		
223.3			227.5		
225	224.0	1.3	225	226.2	1.5

TABLE VI.

SUBJECT Fs.

	High Tone. Av.	M. V.		*Low Tone.* Av.	M. V.
290*			101.5*		
310			105.9		
311.2			110		
310			114.1*		
305	304.4	6.6	115	109.3	4.4
310			110		
312.5			111.2		
314.6			112.3		
310			109.7		
315.2	312.4	1.9	111.2	110.8	0.8
322.1			111.5		
322.4			112.5		
326.5*			110.9		
320			111		
323.3	322.8	1.6	112.1	111.6	0.5
318.6			110.6		
320			112.5		
325			111.7		
316.6			111.7		
325	321.0	3.1	110	111.3	0.8

Medium Tone.

207*			222.6		
216.2			220.5		
223.3			223.3		
223.3			222.3		
222	218.3	5.9	222	222.0	0.7
220.5			221.3		
220			221.1		
222			225*		
220			222.1		
221.7	220.8	0.9	224.6	222.8	1.5

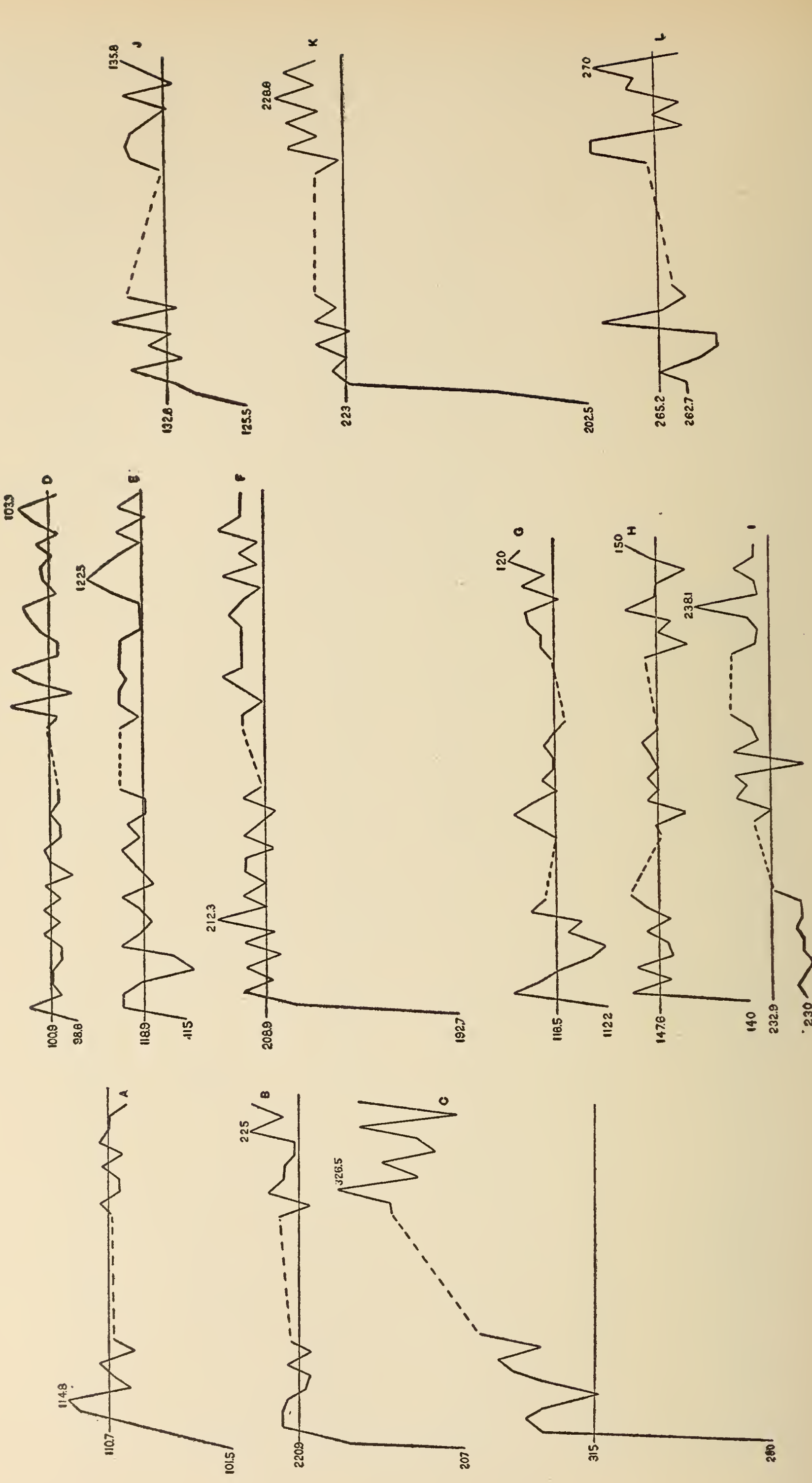
A
114.8
110.7
101.5
B
225
220.9
207
C
326.5
315
280
D
103.3
100.9
98.8
E
122.5
118.9
115
F
212.3
208.9
192.7
G
120
116.5
112.2
H
150
147.6
140
I
238.1
232.9
230
J
135.8
132.6
125.5
K
228.8
223
202.5
L
270
265.2
262.7

FIG. 75.

The most striking feature of the results is, of course, the lack of uniformity of pitch in each of the tones. The lack of uniformity shows itself in three ways: (1) In a marked period of disturbance at the beginning of the tone shown in a high mean variation. This disturbance is not uniformly present but, taken in connection with results to be reported later, it may be considered as quite typical. It usually occurs within a period of three tenths second from the beginning of the tone, and is characterized usually, though not invariably, by a marked rise in pitch during the second tenth of a second over that of the first period of the same length. In only one case out of the twenty-one recorded above is there an actual decrease in pitch at this point in the tone. This occurs in Cg.'s high tone (Table IV.). One or two other cases occur in which the rise of pitch is not marked, but in the great majority of cases there is a rise in pitch quite extraordinary as compared with differences in pitch elsewhere in the tone. This rise is so general a feature of the beginning of a tone, as shown by these and numerous other records made in connection with this investigation, as to warrant the statement that it is a universal tendency.

Out of the thirty-five records not reported in full, taken from three different subjects for the express purpose of testing this point, there was only one in which there was a lowering in pitch during the second tenth of a second. In this case the difference was slight, while in the majority of the cases of the usual type there is a very marked difference.

Plainly, the system of measurement by tenths of a second may result in some cases in covering up the fact of such a rise, since there follows after the rise a lowering of the pitch usually appearing in the third tenth of a second. If the lowering occurs somewhat sooner than the third tenth of a second, it will then be included in the measurement of the second and disguise the fact of the real rise.

The following table gives the results of the measurement of the beginnings of eight tones in periods of twentieths of seconds, the time line having been obtained from a vibrating 100 V.D. fork.

TABLE VII.

Subject Fn.

I.	III.	V.	VII.
245	152	300	95.6
273.2	162.2	303.8	102.6
301.1	174.6	310	106.6
300	160	310	105.2
281.7		310	
		307.5	
II.	**IV.**	**VI.**	**VIII.**
140	150.6	290	109.6
154.8	171.6	300	114.2
160	183.2	300	116.9
168.5	188.2	300	112.4
166.2	186.6	297.4	

It will be seen that the point at which the pitch begins to decrease may be within the second tenth of a second, as shown in the first and last of these tones. Had the latter, for instance, been measured by tenths instead of twentieths of seconds, the difference between the pitch of the beginning of the tone and the highest pitch would have been 2.7 as against 7.3 vibrations. In view of the well-known fact that, in the training of the voice a correct attack is one of the most difficult things to cultivate, these results have been extended to make the matter as clear as possible. The fact that the rise in pitch appears even in the singing of subjects who have had special training seems to indicate that it is due to a physiological factor not under the control of the singer.

In the second place there is noticeable in the tables a tendency to make the latter part of the tone higher than the first. While this tendency is not universal, it seems to be typical in the case of the subjects that have been examined. Ce.'s low tone and Cg.'s high tone are the only exceptions. The tendency to sharp the latter portion of the tone is indicated in the curves of Fig. 75, page 256, and in the relative position of the starred figures in Tables I.–VI.

The third characteristic is the lack of uniformity in the record of the succeeding periods of each of the tones. It is a rare occurrence for the number of vibrations to be the same in any two successive periods of one hundred sigmas, though the pitch of some of the periods at the beginning of Fn.'s high

tone, Table II., p. 250, are quite remarkable for their uniformity as compared with the other records of this series, as well as those which were obtained later. This marked irregularity in the maintaining of any tone is apparent in all the records examined. The time involved in the changes of pitch is so short and the changes themselves so small as to preclude the probability of their being reactions of a fully conscious kind.

Involuntary movements of a similar kind are found to be present in other forms of motor adjustment under analogous conditions. The eye, for instance, in fixating a point, does not do so exactly, but oscillates more or less in various directions about the point.[1] So, too, in the state of muscular tension preparatory to the signal for reacting in reaction-time experiments, the subject's hand does not maintain exactly the same tension, but moves upward and downward in a somewhat rhythmical fashion.[2]

The rhythm of muscular response in the case of voluntary and reflex contraction has been made the subject of much study. It has been variously stated by investigators that the rate of such rhythm of response is from 8 to 20 per second. The tables show that in the case of the muscles used in control of the pitch of the human voice the rhythm is by no means a regular one. In all of the records the measurements were made by tenths of seconds, but while some of them show a somewhat marked rhythm for this period, for the most part the changes in pitch from higher to lower or lower to higher occur at irregular intervals.

Of course, this might be due to the fact that the period of the rhythm and that of the time line do not exactly correspond. In order to test this point somewhat more carefully, a number of records were taken, in which the time line was obtained from a vibrating fork of 100 V.D. frequency. In this way a few measurements were made of each succeeding 10 sigmas of certain tones. The results for three different tones given in the

[1] Cloyd N. McAllister, 'The fixation of points in the visual field,' *Yale Psychological Studies,* New Series, Psychological Review Monograph Suppiement, Vol. I., No. 1, p. 17.

[2] Judd, McAllister and Steele, 'Analyses of Reaction Movements,' *Yale Psychological Studies,* New Series, Psych. Rev. Mon. Sup Vol. I., No. 1, p. 141.

number of vibrations occurring during one hundredth of a second are as follows:

I. 1.76, 1.58, 1.76, 1.64, 1.58, 1.61, 1.62, 1.68, 1.75, 1.64, 1.68.
II. 3, 3.17, 3, 3, 3.25, 3.12, 3.12, 3, 3.06, 3, 3, 3, 3.18.
III. 2.50, 2.33, 2.22, 2.28, 2.50, 2.42, 2, 2.28, 2.14, 2.42, 2.28, 2.42, 2.25, 2.37, 2.12, 2.37, 2.37.

An examination of these figures shows that it is only in a very rough sense that the oscillation period throughout the course of the tone can be said to be rhythmical. The fact that these oscillations occur in the singing of the more trained subjects exactly as in the records of the others would seem to indicate that they are not to any great extent under conscious control.

Series II. Maintaining Tones With Short Periods of Interruption.

The second series of experiments consisted in the singing of tones as in the first series, except that each tone was interrupted by short intervals, during which the singing ceased. The object of the experiments was to see how uniformly the subjects would maintain a tone under such conditions. The average interval was three-tenths of a second and is indicated by a double horizontal line in Tables VIII. to XI. which follow. The results really represent tones of very short duration separated by short intervals. They, therefore, should be expected to be similar in character to the beginnings of tones of the earlier series. That the beginning of a tone is a period of adjustment, in which more or less irregularity of pitch occurs is here even more apparent than before. In this series, as well as the first, the introspective records generally indicate the subject's confidence in having sung the tones uniformly.

TABLE VIII.

Subject J.

Medium Tone.		*Low Tone.*	
165	148.8	103.1	121.7
169.8	162.5	122	129.1
171.5	170	127.3	130
173.3	177.2	130	131.3
172.6	172.5	127.2	130
169.5	144	118.2	120
169.3	165	125	128.1
168.5	164.8	126.3	128.3
171.5	170	128.5	129.7
173.5	168.7	130	130
161.2		120	
170		128.5	
170		129	
170		131.5	
170.9		130	

High Tone.

235	230	230	240	220
235	230.8	240	240	234.5
238	230	233.1	235	235
235	231	237.8	235	234.5
235.7	235	235	235	240

TABLE IX.

Subject Fn.

Medium Tone.		*High Tone.*	
133.8	140	188	192.3
143.8	145	198	200
140	143.1	200	200
140	143.1	204.6	200
142	143.7	206.2	204.3
135	140	201.1	200
141.8	143.3	203.1	204.8
140	140	205.5	203.3
141.1	143.3	210	204.1
141.1	143.7	210	208.2
139		192.2	
145		200	
143.5		200	
141.9		203.9	
140		202.1	

Low Tone.

105.6	101	102.8	98.2	102.8
108.1	102.1	103.3	101.9	104.2
104.2	101.7	102.6	102.5	104.4
104	101.7	103.3	103.7	105.7
103.5	103.2	103.4	104.2	104.4

TABLE X.

SUBJECT CE.

High Tone.		*Medium Tone.*	
121	122.2	120	113.3
127.6	135.2	123	125
133.5	140	124.2	124.7
134.7	140.5	124.4	125.6
135.5	139.5	124.2	125.5
122.2	130	123.5	118.1
124.4	133.8	130	124.6
135	140	125	125.4
134.1	141.5	126.6	125.8
135.8	140	124.4	124.2
125		112.9	
135.2		120	
137.5		120	
137		126.6	
136		127.1	

Low Tone.

77.4	75	74.6	77.8	80
80	74.1	74.4	80	75
78.5	77	72.1	78.9	77.5
77.5	80	77.3	80	80
79.2	79.3	78	78.9	78.8

TABLE XI.

High Tone.

224.2	232	230	230
230	235	233.3	234.2[1]
230		230	

Medium Tone.

174.6	183
190	193.3
190	193.3
190	194.2
185	174.6
191.8	186.4
190	188.7
190	190
183	180
193.3	193
193.3	195
194.2	195
	194.5

Low Tone.

160	157.7
170	166.4
170.8	170
170	166.8
170.5	167.5
169.4	168.2
170	170
170	166.3
172.1	170
173.3	171.4
154.2	
170	
171	
170	
170	
167.8	

[1] Record incomplete.

Series III. Imitating Tones From Organ Pipes.

After these preliminary tests of a subject's ability to maintain a tone chosen at random, and to reproduce a tone after short intervals, a long series of experiments was made for the purpose of discovering the ability of one of the subjects (Fn.) to imitate the tones of organ pipes, varying in pitch from 93.7 to 302.8 vibrations per second.

These experiments were carried out in the following manner. When the subject was ready, one of the keys (*E*), Fig. 5, was pressed down, allowing the pipe, which was to be imitated, to sound for about five seconds. Immediately after the pipe had ceased sounding the reactor sang a tone which was designed to be of the same pitch as that of the pipe. After singing one tone in this way, he wrote upon a slip of paper provided for the purpose his impression concerning the exactness of the sung tone and any other features of the performance revealed by introspection.

In these experiments three sets of results were obtained. The first set, Table XII., was obtained from giving the tones from the pipes in a particular order, viz.: beginning at D (144.2 vibrations per sec.) and proceeding to D♯ (152.4 vibrations per sec.), and so on by regular intervals of half a tone to D♯; thence beginning at C (135.6 vib. per sec.) and proceeding down the scale in a similar manner to G (93.7 vib. per sec.).

In the second set, Table XIII., no particular order was followed, but the experimenter passed from one part of the key-board to another, the effort being made to prevent any expectation on the part of the subject of the direction of the pitch of the tone, as compared with that just previously given.

The last set of results, Table XIV., was obtained in the same manner as those just mentioned except that, in the latter case, the organ pipe which was being imitated was kept sounding while the subject was singing.

The figures given in these tables represent the first and last half seconds of the tone only. Other parts of the tones are omitted for the sake of economy of space. The number of

vibrations per second of the standard tone produced by the pipe is given at the top of each column, and also the letter representing that tone in the usual notation. A space between the figures indicates that the figures above the space belong to the first part of the tone, while those below the space belong to the last part of the tone.

TABLE XII.

STANDARD TONES GIVEN CONSECUTIVELY.

Subject Fn.

G — 93.7.	G♯ — 100.	A — 107.1.	A♯ — 112.1.	B — 120.	C — 126.6.
101.2	96	108.7	112.7	122.3	132
110	108.6	110	118.7	120	133.7
104	110	110	120	118.2	132
104	110	111.2	121	120	131.1
106	113.3	112	120	122.5	130
103.5	115	112.1	115.7	121	131.1
101.3	113.3	110	116.6	121.5	132.9
105	115	110.5	118.1	120	132.5
107.1	114.6	110	118.1	121.5	130
110	113.3	108.7	115.8		130

C♯ — 135.6.	D — 144.2.	D♯ — 152.4.	E — 160.	F — 170.	F♯ — 178.1.
162.5	137.5	150	144.1	148.5	173.3
145	145	152.8	160	170	180
138.5	145	155.7	160	168.5	180
140	146.6	152	160	170	180
138.5	144.8	153.1	161.9	170	180
140	150	155	163.1	172.8	182.5
139.1	145	154	162.8	172.3	182.8
140	147.5	152.6	161.7	172.3	183.1
140	146.6	155	160.8	172.3	180.5
140	147	157.3	163.3	172.3	182.5

G — 187.	G♯ — 200.	A — 214.	A♯ — 225.	B — 240.	C — 255.4.
190	205	195	227.5	235	255
192.7	210	210	242.5	247.5	256.2
191.4	210	215	232.5	247.5	260
191.4	207.5	215	232.5	250	265
193.5	207.5	215	232.5	248.5	265
195.4	206.4	223.3	240	245	260
196.6	207.5	222.8	232.5	245	260
196.3	208.3	225	232.5	245	261.4
197	210	220	235	247.5	260
198.1	208.1	222.8	235	245	260

C♯ — 271.8.	D — 286.4.	D♯ — 302.8.
260	270	247.5
272.5	280	275
280	285	297.5
277.5	285	310
278.7	287.5	310
275	285	310
275	289	310
277.5	286.5	305
275	287.5	310
277.9	285	305

TABLE XIII.

Standard Tones not Given in Order.

Subject Fn.

G — 93.7.[1]	G♯ — 100.	A — 107.1.	A♯ — 112.1.	B — 120.	C — 126.6.
	101.3	123.3	118.5	123.5	121.6
	102.6	115	120	124.5	123.1
	101.6	115	120	122.9	130
	101.6	112.1	121.6	123.4	132.3
	103.3	113.3	122	124.5	130
95.8	104	114.1	124.8	122.5	135
96.2	104.2	113.1	121	122.6	134.6
95	102.1	114.1	121	125.3	131
95	105.2	112.3	125	120	132.2
98.7	102.3	112.3	124.3	120	131.5

C♯ — 135.6.	D — 144.2.	D♯ — 152.4.	E — 160.	F — 170.	F♯ — 178.1.
158	155.8	155	167.5	167.5	187.2
160	156.8	160	167.5	177.2	190
140	150	155	170	177.2	183.3
135.3	152	154	168.3	175.4	183.3
135.6	150	154	165	177.2	184.2
137.7	150	153.5	164	175	186.7
133.3	150	153.5	163	175	188.5
136.5	150	153.5	165	175	184.8
136.1	150	154.6	165	175	182.3
135.9	150	151.4	164.2	178.7	184.5

G — 187.	G♯ — 200.	A — 214.	A♯ — 225.	B — 240.	C — 255.4.
190	195	215	250	225	220
200	205	217.5	247.5	240	252.2
199.2	202.5	220	237.5	232.5	250
197.5	202.5	218	227.5	240	255
195	207.5	220	222.5	240	255
198.5	205	220	227.5	245	252.3
197.5	210	220	227.5	242.5	252.3
195.3	207.5	222.5	222.5	240	252.3
197.1	207.5	220	227.5	240	255
198.7	207.5	222.5		241.9	252.3

C♯ — 271.8.	D — 286.4.	D♯ — 302.8.
232.5	280	242.5
255	282.8	277.5
262.5	282.8	295
271.2	287.1	305
270	287.1	307.5
272.5	286.7	305
274.5	283.3	310
272.5	284.4	315
273.7	283.4	310
273.3	286.6	305

[1] Record illegible.

TABLE XIV.

Given Irregularly with Pipes Sounding.

G — 93.7.	G♯ — 100.	A — 107.1.	A♯ — 112.1.	B — 120.	C — 126.6.
104.7	102.8	109.3	114.4	122.1	130
101.2	95.2	110	115.7	121.6	132.8
101	100	109.3	116.8	120.5	130
101.9	100	111	118.2	120	130.9
100	100	111.8	118.3	118.9	133.1
97.5	103.3	110.9	116.9	124.8	133.3
97.1	101	110	116.7	122.6	133.1
100	104	110.9	117.5	122.5	133.2
99.5	102	110	117.4	122.5	131.6
98.5	104	110	116.5	121.5	130

C♯ — 135.6.	D — 144.2.	D♯ — 152.4.	E — 160.	F — 170.	F♯ — 178.1.
133.5	130	150	178.5	172.5	171.4
143.3	139	152.5	180	175	179.5
137.7	144	153.2	168.2	175.3	179.5
133.6	145.2	150.5	162.6	172.1	180
133.3	147.3	150.9	164.5	173.1	181.5
134.5		153.4	160	174.2	183.1
134.5		152.7	161.5	171.1	181.6
135.4		151.3	161.5	173.3	180
134.2		150.8	161	173.3	182.5
135		150.5	165	175.8	180

G — 187.	G♯ — 200.	A — 214.	A♯ — 225.	B — 240.	C — 255.4.
270	222.5	216.2	249.1	154.2	196.5
258.7	230	212.5	240	199.2	212.5
232.5	212.5	212.5	230	217.5	245
220	206.8	216.7	232.5	242.5	266.2
210	205	216.2	230	252.5	274
205.2	205	220	230	240	270
205	205	217.5	227.7	241.5	272
205	205	220	227.1	242.5	270
200	202.5	220	227.1	242.5	272.3
204	204.4	220	225	242.8	273.8

C♯ — 271.8.	D — 286.4.	D♯ — 302.8.
271	260	285
287.2	280	292.5
271.5	285	307
287.2	285	309
271.5	286	308
272	286.2	305.5
272	288.3	303.3
272	283.8	303.3
272.5	288.3	302.2
270	284.4	304.4

The above series of tables present complete results for twenty-one tones from one subject (Fn.). In the case of the other subjects, a less comprehensive series of tests was made, the number of tones for each person having been reduced to three. One of these three tones was of high pitch, one of low pitch, and one of medium pitch.

The medium pitch given for imitation was in each case E = 160 vibrations per second, but the high and low pitches varied with the natural range of the singer. J. and Ce. sang each of the three tones both with and without the simultaneous sounding of the standard tone; Cg. and P. gave records of the three tones under the latter conditions only, that is, after the pipe had ceased sounding. The tables presenting the results for these four subjects follow (Tables XV.–XVIII).

TABLE XV.

SUBJECT J.

After Pipes had Ceased Sounding.

C — 255.4.	E — 160.	C — 126.6.
245	164.7	131.7
264.2	172.5	132.3
260	172.9	131.3
264.5	176	133.5
265	172.5	132.7
258.5	181	135.1
255.8	181.1	135
257.6	181.8	137.6
261.5	179.5	132.6
261.5	182.5	131.6

With Pipes Sounding.

C — 255.4.	E — 160.	C — 126.6.
240	215	118.7
260	217.5	120
253.1	216.2	118.8
250	215	123.1
250	217.5	123
257.5	218	128.6
260	217.5	127.7
257.5	217.5	130
260	217.5	128.9
258.7	217.5	127.5

TABLE XVI.

SUBJECT CE.

After Pipes had Ceased Sounding.

D — 286.2.	E — 160.	A — 107.1.
280	159.2	97.7
285	167.5	110
290	160	107.3
290	162.5	105
288.5	162.5	105.6
290	160	106.1
293.6	161.2	107.8
293.3	160	106.5
290	160	107.2
293.8	164.1	107.5

4

With Pipes Sounding.

D — 286.2.	E — 160.	A — 107.1.
282.1	158.5	105.6
292.5	160	106.4
285.3	159.5	106.6
287.8	161.7	106
290	160	105
288.5	159.5	107
290	160	106.3
291.3	160	107
291.3	159.5	107
291.3	159.5	105

TABLE XVII.

Subject Cg.

After Pipes had Ceased Sounding.

C♯ — 271.8.	E — 160.	C — 126.6
271.8	160	135.2
275.6	162.1	135.2
271.2	164.4	136.4
278.8	164	134.6
273.6	163.2	132.9
275.2	163.7	136.3
277.5	163.3	134
274	165.6	134
276.4		137.5
276.4		137.2

TABLE XVIII.

Subject P.

After Pipes had Ceased Sounding.

D♯ — 302.6.	E — 160.	A♯ — 112.1.
232.6	146.6	104.2
250	160	105
282.5	162.8	112.9
300	170	112.7
306.6	161	115
304	162.5	117.5
308.5	160	120
302	162.2	115
301.5	165	120
301.1		114.6

The results presented in these tables are similar in certain general characteristics to those of the earlier series made without the use of pipes. Of the eighty-one tones, figures for which are given, only ten show a decrease in the pitch during the second tenth of a second. Of these ten there are three in which the difference in pitch is less than three vibrations, an amount which is quite insignificant as compared with the usual

large amount of increase in pitch during the same period. In the remaining seven cases the beginning of the tone is considerably higher than the standard and the decrease in pitch during the second tenth of a second is merely a part of a general lowering of the tone to make it conform to the standard.

An interesting instance of this kind is afforded by the figures given for C—135.6, Table XII., page 264. It will be remembered that this is the point at which the change was made in the order of the standard tones. Up to this point the subject had been singing successively higher tones, until the upper limit of the experiment (D—302.8) had been reached. The experimenter then went back to the tone next lower than the tone with which the series was begun. That the expectation created by the manner of procedure, either in the form of a mental tendency or a motor adjustment, had its effect is clearly shown by the record. Instead of the beginning of the tone being considerably lower than the standard tone and the immediately following part much higher, as is usually the case, the beginning is much higher than the standard and the tone at once begins to drop in pitch, so that the larger part of the tone becomes a fair approximation of the standard. The introspective record does not show that the subject was conscious of the character of the early part of the tone. In other cases where there is a marked change of pitch in the early part of the tone there is sometimes a recognition of the fact after the singing has ended, as shown by the introspective records, but usually this is not the case.

Take, for example, the record for C—255.4 vib., Table XIV., page 266, in the last of the series under consideration, where the pipe was sounding while the singing was going on. The subject noted that the beginning of the tone was much too low, and ascribed the trouble to a difficulty he felt in motor control. He failed to note, however, that in overcoming the flattening at the beginning, he went too far in the other direction, making the tone as a whole higher than the standard, and to a degree which is quite uncommon in other cases where he sang tones of that range.

As was to be expected, the introspective records show that

the subject is more confident in his judgments as to the success of his singing in the experiments where the pipe was kept sounding. An important point which is brought out by the introspection of Fn. in connection with the later series, relates to the direction of the attention during the singing. Sometimes the attention was more fully centered upon the auditory stimulus from the sounding pipe, sometimes upon the singing of the tone. No different results were noted, however, corresponding to these different directions of the attention.

Another point demanding fuller study is the effect which the singing of a tone has upon a following tone. An instance of this kind is the one mentioned above, where the effect of singing a series of progressively higher tones led to a failure in adjustment at the beginning, when a lower tone was given as the standard. The following table, Table XIX., gives some results which are interesting in this connection. These results were obtained from the measurements of some records previous to the perfecting of the apparatus for measurement and, therefore, may not be as accurate as the other results.

The subject, Mr. W. M. Steele, was asked to sing two tones, A—219.5 vib., and C—265 vib., in succession, with a short pause between. This was done three times. He was then asked to sing the tones in the reverse order three times. The standard tones were given by means of a pitch pipe in the order they were to be sung.

The table follows:

TABLE XIX.

A — 219.5	224	217	222	...	220	222	222
C — 265	277	270	270				
A — 219.5	225	220	220	...	227	228	223
C — 265	268	269	265				
A — 219.5	200	220	216				
C — 265	225	221	255	280	269	270	
C — 265	265	265	269	...	270	270	261
A — 219.5	201	209	200	209	210	210	210
C — 265	280	270	266	...	270	270	260
A — 219.5	218	212	215				
C — 265	278	266	268	...	271	280	270
A — 219.5	200	210	212				

It will be seen that where A precedes C, it is always to a greater or less degree higher in pitch than the standard. On the other hand, when A follows C it is invariably lower than the standard.

Other instances of the effect of a tone upon an immediately succeeding tone might be taken from some of the results when the subjects had little or no 'ear for music.'

One subject, Dr. C. M. McAllister, who had enough musical development to sing an air with others, and who could discriminate different pitches fairly well, was asked to sing any tone he wished, taken at random. The tone sung averaged 221 vib. per sec. He was then given a standard tone of 193 vib. per sec., and asked to imitate it. He responded with a tone of 231 vib. per sec. Holding to 193 vib. per sec. as the standard, he was asked to sing it, and other tones were used as distractions. Three tones sung under such conditions averaged 230, 231 and 222 vib. per sec., respectively. To what extent the failure to sing a tone correctly under such conditions is due to lack of the powers of auditory discrimination and to what extent it is due to lack of motor control, is a question which might be investigated with profit.

Another subject, Mr. Browning, when given a tone from a pipe to imitate, would fairly approximate the standard. Succeeding tones, however, were all sung at practically the same pitch if they were anywhere within the same range. It was only by changing the standard to one much higher or lower than the first given that any change in the pitch of the sung tone could be obtained.

In order to show how nearly the tones were approximated in each case the figures of Tables XII. to XIV. have been averaged (Table XX., page 272) and the error computed as a percentage of the number of vibrations of the standard tone. Since, however, the beginning of the tone is, as has been seen, a period of adjustment, the average of the first five measurements was not considered in these computations. The average tone is more adequately represented by the average of the remaining part of the tone, since the first average in the table represents but a comparatively small portion of the tone.

Where the average tone is higher than the standard, this fact is represented by the positive sign, and where it is lower the negative sign is used to indicate that fact. Table XX. gives the per cent. of error for each of the tones as sung under the conditions already described.

TABLE XX.

PERCENTAGE OF ERROR.

Subject Fn.

Tone.	No. Vib.	Standard Given in Order of Pitch.	Standard given in Irregular order.	Pipe Sounding.	Average for each Tone.
G	93.7	+ 11.9	+ 3.65	+ 5.0	6.8
G♯	100	+ 13.6	+ 3.6	+ 1.6	6.2
A	107.1	+ 3.5	+ 4.8	+ 3.4	3.9
A♯	112.1	+ 5.3	+ 8.3	+ 5.6	6.0
B	120	+ 0.8	+ 2.1	+ 1.6	1.5
C	126.6	+ 3.4	+ 3.3	+ 4.4	3.7
C♯	135.6	+ 3.1	+ 0.2	— 0.3	1.2
D	144.2	+ 1.7	+ 3.7	+ 0.3	1.9
D♯	152.4	+ 0.5	+ 1.4	— 0.3	0.7
E	160	+ 3.3	+ 2.8	+ 1.4	2.5
F	170	+ 1.4	+ 2.9	+ 2.0	2.1
F♯	178.1	+ 1.8	+ 4.0	+ 2.6	2.8
G	187	+ 4.8	+ 4.9	+ 9.0	6.2
G♯	200	+ 3.3	+ 4.0	+ 2.8	3.3
A	214	+ 4.3	+ 2.7	+ 2.3	3.1
A♯	225	+ 3.8	+ 0.7	+ 1.1	1.8
B	240	+ 2.1	+ 0.04	+ 0.8	0.9
C	255.4	+ 1.6	— 0.5	+ 6.3	2.8
C♯	271.8	+ 1.6	+ 0.2	— 0.4	0.7
D	286.4	+ 0.4	— 0.2	— 0.5	0.3
D♯	302.8	+ 1.7	+ 1.4	+ 0.5	1.2

Tone	No. Vib.	After Pipe.	Pipe Sounding.
Subject J.			
E	160	+ 13.0	+ 3.5
C	225.4	+ 1.2	+ 0.5
C	126.6	+ 6.2	+ 0.3
Subject Fs.			
E	160	+ 0.2	
D	302.8	— (100 + 0.2)	
A	107.1	+ 1.9	
Subject Ce.			
E	160	+ 0.6	+ 0.3
A	112.1	— 0.8	— 0.3
D	286.2	+ 1.8	+ 1.3
Subject Cg.			
E	610	+ 1.3	
C	126.6	+ 6.2	
C	271.8	+ 1.8	
Subject P.			
A	112.1	+ 4.7	
E	160	+ 1.0	
D	302.8	+ 0.7	
Subject G.			
E	160	+ 1.0	
B	240	+ 1.2	

It will be seen that the per cent. of error varies all the way from 0.04 per cent. to 13.6 per cent. Klünder[1] found an error of only a small fraction over 3 per cent. to be the maximum in the subjects he examined. In making his computation, however, he rejected all those records which showed unusually large errors. Moreover, his subjects were chosen because of their musical training and ability. It should be remembered also that the conditions of Klünder's experiment made it necessary that the record from the voice and organ pipe should be taken simultaneously. This probably aids in exactness, though the figures of Table XX. are not very conclusive on that point. If we except the records for G—187 vib. and C—255.4 vib., the gain in accuracy when the pipe is sounding is more marked. Both of these cases evidently come under the class of records rejected by Klünder. The introspective record shows that the subject knew of his error and it seems fair to conclude that unknown distracting conditions were present in these cases, resulting in a lack of motor control which he could no doubt correct if given another opportunity.

By far the greater error occurs in connection with the low tones. This is in accordance with the subject's own judgment as to his ability in discriminating these tones. He experienced much more difficulty and confusion in discriminating low tones in general, and felt less certainly that he had correctly imitated them. The higher tones, except in the cases just mentioned, were very closely approximated. These remarks are true, not only of Fn.'s results, but also of those of the other subjects examined except Fs.

The most marked feature presented by Table XX. is the almost universal presence of the plus sign indicating a higher tone than the standard. Fn.'s tendency is plainly to sing a tone higher than the standard which he is endeavoring to imitate. In only six cases out of the sixty-three does he sing the tone lower than it should be sung. In these six cases the amount of error is extremely small. Four of these occur in the singing of the higher tones, thus making it appear that the

[1] 'Ueber die Genauigkeit der Stimme,' *Arch. f. Anat. u. Physiol.* (Physiol. Abth.), 1879, p. 119.

natural tendency to sharp had been overcome by the difficulty of reaching these higher tones. With the other subjects examined the tendency is also general to sing the tone higher than the standard.

TABLE XXI.

SUBJECT FN.

	Without Pipe Sounding.		With Pipe Sounding.
1	2.3	3.0	2.9
2	3.2	2.8	1.5
3	3.2	3.9	1.7
4	2.9	2.4	1.1
5	2.1	2.5	1.5
6	2.6	4.6	2.2
7	2.5	6.1	2.1
8	2.6	0.8	1.6
9	1.9	3.0	2.1
10	3.0	1.7	3.5
11	2.3	3.4	1.5
12	1.9	3.8	3.6
13	3.3	3.1	3.7
14	2.3	3.2	2.5
15	4.0	3.9	3.2
16	4.6	4.5	4.5
17	2.3	4.2	4.7
18	3.5	3.1	2.9
19	3.7	3.6	2.4
20	4.9	3.1	3.8
21	4.0	5.0	2.7
Av.	3.0	3.1	2.4

SUBJECT CE.

		5.4	4.8
		2.9	1.8
		3.4	1.3
Av.		3.9	2.6

SUBJECT J.

		2.2	1.5
		3.6	3.3
		3.4	2.5
Av		3.1	2.4

Attention has been drawn to the fact that a difference in accuracy in favor of the tones accompanied by the sounding pipe can be noted from the above figures. There is one other way in which the accuracy of the sung tone may perhaps be affected by the actual presence of the standard tone. The tone may be more uniform from period to period during the course of the singing. The foregoing table, Table XXI., is designed

to show the comparative uniformity of the tones. The figures represent the sum of the three mean variations of each of the columns in Table XX. It also shows similar results of tones sung by Ce. and J. with and without the presence of the sounding pipe.

Twenty-seven cases in all are thus represented. In all but eight cases the mean variation is less for the tones with the pipe sounding. But there are two series without the pipe. In only three cases of the eight are the mean variations in the third column greater than both of the others. These figures indicate a slightly greater degree of uniformity for the tones accompanied by the sounding pipe.

Series IV. Effects of Distraction.

The remainder of the investigation relates to the effect of distractions in connection with the singing of tones. The procedure followed was practically the same as in the earlier series except that a distraction (usually a tone from one of the pipes) was used during the singing of the standard tone.

The standard tone which the subject tried to imitate was given as before for the same length of time. Immediately on releasing the key and thus causing the cessation of the tone from the pipe which was used for the standard, a second tone was sounded for the purpose of distracting the subject.

It was planned to begin the distracting tone in this way at the same time or a little before the sung tone had begun, and to continue it while the latter was being sung. Sometimes designedly, and sometimes on account of the slowness of the operator in pressing the second key, the subject began to sing a little before the tone designed to distract him had set in, but in nearly every case the distracting tone had begun before the singing of the tone commenced.

The distracting tones were distributed so as to test the influence which might be made upon the singing by the simultaneous sounding of tones which were (1) harmonious or inharmonious with the standard tone; (2) of greater or less interval from the standard; and (3) higher or lower than the standard.

In a few cases the form of distraction used was a sudden loud noise caused by the stamping of the foot upon the floor at the moment when the singing was about to begin.

In the manner just described, a series of records was obtained of tones sung under different conditions of distractions. In the following tables, as in the preceding, the figures for the first half second and last half second only are given.

Whenever it was thought probable that other portions of the record might show signs of distraction, these were also read; but disturbances when they occurred almost invariably showed themselves at the beginning of the tone, or in the pitch of the tone as a whole.

Records of the singing of thirteen tones under conditions of distraction were obtained from subject Fn., and these all appear in the following tables. In the case of the other subject three tones, one of high pitch, one of low pitch, and one of medium pitch were in general used. It is not thought necessary to indicate the full series of figures from the records of these latter subjects. The percentage of error and its direction only are given in some cases where the disturbing influence is apparent throughout the whole of the record. In a few cases where no effect of distraction is apparent, that fact is indicated without the numerical statement.

The pitch of the standard tone is indicated at the top of each table, and the distracting tone is also given at the top of the column of figures indicating the results of the tone sung, when that particular distracting tone was used.

The tones are given in the order in which they are sung, but this order is not considered significant, as there was a sufficiently long interval after each experiment, to preclude the possibility of any influence lasting over from one tone to the next. The figures for the whole series of tones sung under conditions of distraction are presented in Tables XXII. to XXVIII. Tables XXV. to XXVIII. indicate the results in an abbreviated form, either by stating the fact that no distraction occurred or by giving the percentage of error and indicating its direction by the positive or negative sign.

TABLE XXII.—SUBJECT FN.

Standard F — 170.

G — 187.	D♯ — 302.8.	G — 93.7.	D♯ — 152.4	Stamp.
151.2	167.1	152.3	153.4	152.5
160	172.8	163.1	174.1	165.6
163.7	168.8	167	168.5	170
166.6	170	170	170	172.3
167.7	170	173.3	173.1	160
167.2	173.5	171.5	173.9	170
167.2	174	171.5	176.1	170
167.2	173.5	170	175.2	170
166.6	174.2	171.5	174.3	168.9
166	175	170	171.7	

Standard C — 126.6.

C♯ — 135.6.	E — 160.	C♯ — 271.8.	E — 80.
126.6	120	130	182.7
130	125	128.1	200.2
135.8	130	129.5	207.5
137.2	130	129.5	205
134.9	130	130	207.5
133.3	130	132.6	210
135.2	141.1	132.8	210
135.2	130	131.4	210
137.1	131	132.5	210
136.5	130	132.5	205

Standard B — 120.

C♯ — 135.6.	D — 144.2.	C♯ — 271.8.	D — 286.2.
114	130	115.4	122.1
120	120	115.8	124.5
110	120	119	124.5
110	120	119	123.4
110	120	120	120.6
118.8	122.2	122	124.5
116.2	121.5	122.9	124.2
120	120.9	121.3	123.5
120	121.8	122.6	122.1
118.1	125	121.2	120

Standard G♯ — 200

Stamp.	A♯ — 225.	C — 255.4.	A♯ — 112.1.	C — 126.6.
174.6	190	190	232.5	200
196.8	207.5	207.5	235	197.5
200	203.4	205	220.2	200
198.7	205	205	215	200
198.7	207.5	200.2	217.5	200
207.5	205	205	217.5	204.3
200	205	205	220	211.5
	207.5	207.5	220	206.5
	205	205	220	205
	205	205	220	205

Standard A — 107.1.

C — 126.6	Stamp.	C♯ — 135.6	C — 255.4.	C♯ — 271.8
100.8	117.6	100	114.1	115
106.7	112.4	110	112.3	115.5
114.7	110	110	111	114.6
108.5	112.5	110	112.2	113.2
108.5	112	110	112.8	112.4
107.5		108.9	112.5	112.4
108.7		110	111.2	115.2
108.7		108.5	110.8	114
108.7		110	110	114
106.2		108.1	110	113.5

Standard E — 160.

E — 80.	F — 170.	C — 126.6.	C — 255.4.
150	151.3	156	141.5
155.2	160	164	158
153.3	158	164.4	160
156	163.5	163.5	162
156	164.1	164.1	160.5
160	162.4	164.5	160.4
158.6	162.6	164	161.3
160	161.6	162.2	161.3
158.5		165	161.3
158.5		161	160

Standard C♯ — 135.6.

C — 126.6.	D♯ — 152.4.	C — 255.4.	D♯ — 75.5.
127.5	130	121.8	119.3
134.1	137.5	131.4	122.8
140	138.3	136.1	125
138.5	138.4	136.6	125.8
138.5	140	138.3	129
141.4	138.6	138.5	130
141.2	140	137.5	128
140	137.1	140	128.4
138.8	138.1	140	125.4
	137.1	143	127

Standard A — 214.

C — 255.4.	B — 240.	C — 126.6.	B — 120.
210	205	190	210
217.7	212.7	210	215
220	217.4	212.5	218.7
220	215	210	218.7
220	217.6	217.5	217.5
221.2	220	222.2	221.2
221.2	220	222.3	222.5
217.5	220	220	222.5
217.5	218.5	221.8	220.5
221.2	218.5	221.8	221.2

Stndard D — 144.2.

E — 160.	G — 187.	G — 93.7.	E — 80.
135.5	143	131.1	142.5
145	148.4	144	144.1
144.1	146.4	144	141
145.5	146.2	145.3	141
144.7	145	144.7	144
144.1	146.1	145.2	144.8
144.1	146.2	143.6	145
143.1	146.6	143.6	145.4
142.7	146.1	146.1	145.3
143.1	145	146.1	145.9

Standard G♯ — 100.

D♯ — 75.5.	G — 93.7.	D♯ — 152.4.	G♯ — 200.
100	102.5	107.4	105
100	102.5	105.7	105.3
104.7	102.5	104.3	104.8
102.7	104.2	104.1	105.4
103.6	105.8	103.3	105
104.9	104	105	104.3
105.1	105	105.7	103.2
106	103.9	105	103.1
105.6	105.3	104.5	105
	108.3	106.6	105

Standard D — 302.8.

D — 144.2.	D — 286.2.	B — 120.	B — 240.
171.5	293.3	265	292.4
177.5	312.5	310	304.5
175	310	305	310
177.5	310	305	310
195	312.5	305	310
301.3	312.2	310	310
299.5	310	312.5	308.7
300	310	310	310
303.5	310	307.5	310
305		308.7	310

Standard G — 187.

G♯ — 200.	C — 255.4.	G — 93.7.	C — 126.6
128.3	190	225	174.6
145	202.2	192.3	183.7
167	196.1	181.5	191.7
181.7	192.5	187.5	190
192.5	195	192.2	193.6
197.5	197.1	193.3	195
195.6	195	195	195
195.6	195	195	194
195.6	197.2	193.5	193.3
194.3	192.5	190	194.2

Standard A♯ — 112.1.

C♯ — 271.8.	E — 160.	A♯ — 225.	F — 85.
131.3	111.6	107.5	
137.5	120	118.2	111.2
122.4	115.6	118.2	112
115	115.6	120	113.3
113.1	118.3	118.2	112.1
117.5	117.5	118.4	115.8
117.5	118.5	118.4	115
117.5	118.1	116.1	115
116.2	117.5	118.4	114.8
115.4		118.2	113.3

TABLE XXIII.—Subject J.

Standard C — 126.6.

High sung tone.	C — 135.6.	C — 271.8.	A — 107.1.
192.3	137.5	142.5	141
174.3	137.5	155.3	159.1
175.3	139.1	160	173.6
182.2	138.3	167.4	184.3
187.1	138.7	167.8	194.1
187.3	138.5	170	156.5
187.5	136.6	168.6	156.1
185.5	136.6	166.3	157.7
185	137.5	167.6	154
	136.6	170	155.3

Standard C — 255.4.

C♯ — 271.8.	D♯ — 302.8.	D♯ — 152.4.	A — 112.1.
242.2	232.7	257.4	250
251.6	250	280	250
252.4	260	285	247.8
256.5	269.1	290	250
265	272.4	284.2	249.3
275	310	304.7	
275	310	309.4	
278.7	315	310	
275	310	310	
275	314.7	309.1	

Standard E — 160.

F — 170.	C — 255.4.	C — 126.6.	F — 85.
227.5	184	175.5	204
227.5	190	181	205
232.5	196.1	185.8	204
233.7	200	188.1	208.7
232.5	203.3	186.6	205
240	198	188.7	205
240	197.7	190	205
236.6	200	190	207.5
235	198.6	190	205
242.5	200	192.5	207

TABLE XXIV.—Subject Ce.

Standard E — 160.

Stamp.
No. dis.

F — 85
No. dis.

C♯ — 135.6

128.5
127.1
126.8
128
128.1

126.3
128
128.3
126.6
123.3

D♯ — 302.8
No. dis.

D♯ — 152.4
No. dis.

C♯ — 135.6
No. dis.

D♯ — 152.4
No. dis.

D♯ — 75.5
No. dis.

A♯ — 112.1
No. dis.

Standard D — 286.4.

D♯ — 302.8.	Stamp.	G — 93.7.	G — 187.
292.5	272.7	257.5	272.2
305	315.6	280	283.3
320	307.5	302.8	291.6
316.6	295.8	289.5	296.1
305	292.5	292.1	290
290	292.3	291.2	290
287	295	290	288.8
292.5	282.3	291.3	291.2
283.3	288.7	294	290
287.5	287.5	291.7	290

TABLE XXV.—Subject Cg.

Standard E — 160.

G — 93.7
No. dis.

F♯ — 178.1
No. dis.

C♯ — 271.8
No. dis.

D♯ — 152.4
No. dis.

Standard C — 271.8.

D — 286.4
+ 7.7

C♯ — 135.6
No. dis.

D — 144.2
— 4.1

C — 126.6
No. dis.

TABLE XXVI.—Subject G.

Standard E — 160.

D♯ — 302.8
— 12.1

F — 170
+ 7.6

C — 126.6
— 21.7

D♯ — 152.4
+ 16.3

Standard B — 240.

C — 255.4 No. dis.	D♯ — 302.8 No. dis.
C — 126.6 No. dis.	

TABLE XXVII.—Subject P.

Standard E — 160.

F — 120 + 37.1	D♯ — 302.8 + 33.7
C — 271.8 + 37.9	A♯ — 112.1 + 35.2
F — 170 + 9.7	

Standard D♯ — 302.8.

D♯ — 152.4 No. dis.	E — 286.4 No. dis.

Standard A♯ — 112.1.

C — 126.6 + 15.2	A — 214 + 20.2

TABLE XXVIII.—Subject Fs.

Standard D♯ — 302.8.

D	C	F
— (100 + 1.1)	No. dis.	No. dis

Standard — 107.1.

A♯
— 2.9

Standard E — 160.

F — 170
No. dis.

From an examination of the figures in Tables XXII. to XXVIII., it appears that the distracting tones made an effect upon the pitch of a little less than forty per cent. of cases. Out of one hundred and ten cases recorded, forty-four show clear evidence of the effect of distraction. These are distributed as follows: Subj. Fn., fifteen cases out of fifty-five; Subj. J., thirteen cases out of fourteen; Subj. Ce., two cases out of eight; Subj. G., four cases out of seven; Subj. P., seven cases out of nine; Subj. Fs., one case out of five. Expressed in per cent. the following table gives the relative sensitiveness to distraction:

TABLE XXIX.

Subj.	Per Cent. of Cases in which distraction Occurs.
Fn.	27.2
J.	87.1
Ce.	16.6
Cg.	25
G.	57.1
P.	77.7
Fs.	20

Thus it is seen that the greatest effect of distractions seems to have occurred with J., who is the least trained of all the subjects in musical ability. On the other hand, P., who has had more training in singing than any of the other subjects except Fs., seems only a little less susceptible to distractions than J. Fn. and Cg., who may be roughly classed together with regard to their musical training, are affected by distractions in about the same proportion of cases. G., who belongs approximately to the same class, is disturbed in a much greater proportion of cases. Fs., who has had the advantage of a large amount of training, shows the least disturbance from distraction, with the exception of Ce.

While, therefore, there is a general correspondence between the amount of distraction and the musical training, other influences are apparently present. Since the number of experiments in the case of each individual is not the same, the general tendency may be emphasized even though it is not borne out in every detail. It is interesting to note that Fs., the most trained subject, was the only one to mistake one octave for another. (See Table XXVIII., page 282; also Table XXX., page 285.)

It will now be necessary to state in what ways the presence of the distraction manifests itself. In general it may be said that if the distracting tone has a disturbing effect, the effect is shown in one of two ways. The distracting tone may, in the first place, cause the subject to sing a tone which is much higher or much lower than the standard throughout the whole period. Such an effect is shown in the tables which follow Table XXX., page 285, in a large plus or minus percentage of error. In the second place, the beginning only of the tone may be affected by the distraction while the greater part of the tone is within

the usual amount of variability from the standard for the tone being sung.

In this connection, it may be said that corrections during the course of the singing of the tone are relatively infrequent. Even when the subject knows, as shown by the introspective records, that he is singing the wrong tone, he usually finds it difficult to change the pitch of a tone which has once begun.

The instances in which the error pertains to the entire tone are the most frequent. In such cases a large plus or minus percentage of error appears in the tables. When taken in relation to the pitch of the distracting tone it will be seen that the error is not always in the direction of the distraction. On the contrary, quite a large proportion of cases occur in which there is evidently a tendency to react in a direction opposed to that of the distracting tone.

A good instance of this kind is where Fn. was given a standard tone of 126.6 vibrations, followed by a distracting tone of 80 vibrations, Table XXII., page 277. Instead of the result being a sung tone of less than 126.6 vibrations, there is the large error in the other direction of 64.2 per cent. Again the same subject sang B (120 vibrations) followed by distracting tone of 135.6 vibrations, 3.6 per cent lower than the standard, Table XXII., page 277. When it is considered that the general tendency is to sing the tone higher than the standard, and that this tendency is especially marked in the lower notes, the negative error is the more noteworthy. Other instances of a similar kind occur and with other subjects. The majority of tones in which distractions plainly occur are, however, in the direction of the pitch of the disturbing tone.

In Table XXX., page 285, the results of the foregoing tables are condensed so as to show their main features. There is given the standard tone and the percentage of error when no distraction is present, followed by the statement of the per cent. of error in the tone sung when a distracting tone is used, the distracting tone being indicated in each case above the percentage of error.

Where effects of distraction are apparent they are indicated in three ways, corresponding to the three ways in which

the distraction makes itself felt. Thus the letter $^{\mathrm{X}}$ indicates a tone where the tone actually sung is in an opposite direction from that of the distracting tone; a mark $^{\mathrm{Y}}$ indicates that the sung tone is in the direction of the distracting tone; and a mark $^{\mathrm{Z}}$ indicates that the effect of the distraction appears only in the early part of the tone. The detailed evidence for the conclusions respecting the cases in which distractions are effective appears in the foregoing tables.

TABLE XXX.—Subject Fn.

Standard.	Percentage of Error Without Distraction.	Percentage of Error With Distractions.				
C =126.6	+ 3.3	C♯=135.6 + 6.2$^{\mathrm{X}}$	E =160 + 2.6	C♯=271.8 + 4.5	E = 80 +64.2$^{\mathrm{Y}}$	
B =120	+ 2.1	C♯=135.6 — 3.6$^{\mathrm{Y}}$	D =144.2 + 1.4	C♯=271.8 + 0.8	D =286.2 + 2.5	
G♯=200	+ 4.0	C =126.6 + 2.2	A♯=225 + 2.7	C =255.4 + 1.4	A♯=112.1 + 9.0$^{\mathrm{Y}}$	Stamp —1.6$^{\mathrm{Z}}$
F =170	+ 2.9	G =187 — 1.6$^{\mathrm{Y}}$	D♯=302.8 + 1.8	G = 93.7 + 0.7	D♯=125.4 + 2.3	Stamp +0.5$^{\mathrm{Z}}$
A =107.1	+ 2.7	C =126.6 + 0.1	C♯=135.6 + 2.3	C =255.4 + 4.1	C♯=271.8 + 6.9$^{\mathrm{X}}$	Stamp +0.3$^{\mathrm{Z}}$
A♯=112.1	+ 8.3	C♯=271.8 + 3.7$^{\mathrm{Z}}$	E =160 + 4.6	A♯=225 + 4.7	F = 85 + 2	
D♯=302.8	+ 1.4	D =144.2 — 2.3$^{\mathrm{Z}}$	D =286.2 + 2.5	B =120 + 1.7	B =240 + 2.1	
G =187	+ 4.9	G♯=200 + 4.3$^{\mathrm{Z}}$	C =255.4 + 4.3	G = 93.7 + 4.4$^{\mathrm{Z}}$	C =126.6 + 5.1	
G♯=100	+ 3.6	D♯= 75.5 + 5.2	G = 93.7 + 4.8	D♯=122.4 + 4.8	G =200 + 5.0	
C♯=135.6	+ 0.2	C =126.6 + 2.4	D♯=152.4 + 2.3	C =255.4 + 2.7	D♯= 75.5 — 5.9$^{\mathrm{X}}$	
D =144.2	+ 3.7	E =160 + 0.7	G =187 + 0.9	G = 93.7 + 1.2	E = 80 + 0.4	
A =214	+ 2.7	C =255.4 + 2.1	B =240 + 2.1	C =126.6 + 2.1	B =120 + 3.1	
E =160	3.3	E = 80 — 0.3$^{\mathrm{X}}$	F =170 + 1.6	C =126.6 + 2.0	C =255.9 + 0.9	

Subject J.

C =126.6	+ 6.2	High Sung Tone. +46.8^{X}	C♯=135.6 + 7.8^{X}	C♯=271.8 +33.3^{X}	A =107.1 +24.3^{Y}	Stamp —2.5^{Z}
E =160	+13.0	F =170 +42.2^{X}	C =255.4 +24.6^{X}	C =126.6 +12.3	F = 85 +28.5^{Y}	High Sung Tone. +47.0^{X}

Sang High Tone.

C =255.4	+ 1.2	C♯=271.8 + 7.2^{X}	D♯=302.8 +19.1^{X}	D♯=152.4 +17.5^{Y}	A♯=112.1 + 2.5

Subject Ce.

A =107.1	— 0.8	C♯=135.6 +16.3^{X}	D♯=152.4 No dis.	D♯= 75.5 No dis.	A♯=112.1 No dis.
E =160	+ 0.6	F =170 No dis.	D♯=302.8 No dis.	F = 85 No dis.	D♯=152.4 No dis.
D =286.4	+ 1.8	D♯=302.8 No dis.	Stamp + 1.8^{Z}	G = 93.7 No dis.	G =187 No dis.

Subject Cg.

E =160	+ 1.3	G = 93.7 No dis.	C♯=271.8 No dis.	F♯=178.1 No dis.	D♯=152.4 No dis.
C =271.8	+ 1.8	D =286.4 + 7.7^{X}	D =144.2 — 4.1^{X}	C♯=135.6 No dis.	C =126.6 No dis.

Subject G.

E =160	+ 1.0	D♯=302.8 —12.1^{Y}	C =126.6 —21.7^{X}	F =170 + 7.6^{X}	D♯=152.4 +16.3^{Y}
B =240	+ 1.2	C =255.4 No dis.	D♯=302.8 No dis.	C =126.6 No dis.	

Subject P.

E =160	+ 1.0	F =170 +37.1^{X}	D♯=302.8 + 33.9^{X}	C♯=271.8 +37.9^{X}	A♯=112.1 +35.2^{X}	F=170 +9.7^{X}
D♯=302.8	+ 0.7	D =152.4 No dis.	D =286.4 No dis.			
A♯=112.1	+ 4.7	C =126.6 +15.2^{X}	A =214 +20.2^{X}			

Subject Fs.

D♯=302.8	— 0.2	D =286.4 1.1 (—100)	C =271.8 No dis.	F =170 No dis.
A =107.1	+ 1.9	A♯=112.1 — 2.9^{Y}		
E =160	+ 0.2	F =170 No dis.		

The question now presents itself whether there is any relation between the distracting tone and the tone actually sung in cases where distraction has occurred. In order to show whether such a relation exists, the following table, Table XXXII., is presented. In this table there are arranged the principal cases of distraction of each subject. The standard is given in the usual musical nomenclature and following it the distracting tone and the tone sung. These are also denoted by the letters of the usual musical notation. The relation of these tones to each other with reference to harmony is expressed by the signs = and ∔. Thus B ∔ C# = A# indicates that the standard B was followed by the distracting tone C# which is not in harmony with B, and that the tone sung was A# which is in harmony with the distracting tone. The tones of the lower octave from D# to D# are indicated by small letters, those of the upper octave by large letters.

TABLE XXXI.

SUBJECT FN.

c ∔ c# = c#	b ∔ c# = a#	a = C# = a#
c = E = a	G# ∔ a# = A	

SUBJECT J.

c ∔ c# = c#	E ∔ F ∔ B	C ∔ C# = C#
c ∔ C# = F	E = C = G#	C = D# = D#
c = a = E	E ∔ f = G#	C = d# = D#

SUBJECT G.

E ∔ D# = d#	E ∔ F = f
E = c = c	E ∔ d# = A#

SUBJECT P.

E ∔ F = F	E = C# = A#	a# ∔ c = c
E ∔ f = A	E ∔ a# ∔ A	a# ∔ A = c#
E ∔ D# ∔ A		

SUBJECT CG.

C# ∔ D = D	C# ∔ d ∔ C

SUBJECT CE.

a = c# ∔ c

SUBJECT FS.

a ∔ a# = g

In all, twenty-nine cases are here presented, in which the effects of distractions are plainly apparent throughout the tone.

In twenty cases, or about seventy per cent. of the total number, the distracting tone and the standard tones are discordant. The proportion of discordant distracting tones to harmonious distracting tones actually used was about equal, so that discordant tones are clearly more effective for the purpose of distractions than are harmonious tones.

Moreover, twenty-five cases of the twenty-nine, or eighty per cent., represent instances in which the distracting tone and the tone actually sung are harmonious. Of these nine are cases in which the sung tone is the same as the distracting tone and in two cases it is the octave of the distracting tone. In both these latter cases the sung tones are the octaves of the distracting tones which are nearest the standard.

It might be supposed that in the remaining cases the sung tone would be in the nature of a compromise between the standard and distraction. In only seven cases of the remaining fourteen, however, does this supposition hold good. Nevertheless there is in all of these fourteen cases, as has been seen, a relation of harmony between the distracting tone and the tone actually sung.

Let us now examine those instances in which distraction occurs in a part of the tone only. These will include, in the first place, those cases where the distraction is a tone from an organ pipe and makes its effect apparent in the early part of the sung tone only, and, in the second place, those cases where the noise caused by the stamping of the foot is the form of distraction.

The first group of cases involves corrective movements which invariably take place in the early part of the tone, generally within the first half second. They are, therefore, similar in type to those which have already been described in connection with the series of experiments where no set form of distraction was used.

The beginning of the tone may be in error either in the direction of the distracting tone or in the opposite direction. Thus, Subject Fn. begins to sing standard 112.1 vibrations with distracting tone 271.8 vibrations at 131.3 vibrations, but corrects it after three tenths of a second to 115 vibrations. On

the other hand, standard 302.8 vibrations is begun at 171.5 when the distraction is 144.2 and it is not until after nine tenths of a second that the pitch becomes as high as the standard, though there is a gradual rise in pitch during the whole of this period. The introspective record shows that in both these cases the subject was conscious of the error and its direction. This last remark is true, in general, of the tones under discussion and in this point these tones differ from some of the earlier tones where corrective movements took place.

The effect on the pitch of the tone of the sudden noise caused by stamping the foot is not always marked. It frequently happens, however, as in the first of this kind reported for Fn., that there is a sudden increase in the pitch of the tone about two tenths of a second after the noise. Thus, in this case, the tone increased in pitch from 172.3 vibrations to 180 vibrations and this pitch is maintained for another tenth of a second, when it drops to 174 vibrations and from that point oscillates in the usual manner about the average of approximately 170 vibrations. The sudden rise in pitch is more marked than are the usual variations and are clearly correlated with the entrance of the distraction.

Another point to be noted concerning this tone is the length of the tone. The table presents the figures for the whole tone, which is very much shorter than the tone usually sung. This is characteristic of all the tones sung under this condition of distraction. Moreover, in each such case, the tone as a whole is slightly lower than would be expected under ordinary conditions.

There remains to be reported a brief series of experiments in which it was designed to obtain a surer means of distraction than in the cases mentioned above. Subj. Fn. was asked to sing a certain standard tone. While he was engaged in singing this tone, another tone was sounded for a few seconds only, and at the close of the singing he was required to say whether the latter tone was higher or lower than the standard. It was found that in every case it was possible to state the direction of the distracting tone correctly.

A further complication was now added by asking the sub-

ject to sing the distracting tone immediately after singing the standard. It must be remembered that the distracting tone had been sounded for a very brief period, and that this had taken place while the subject was engaged in singing the standard. Nevertheless, he was able to sing the standard tone correctly, as may be seen from Table XXXII. Further, he was able later at least to approximate the distracting tone, though with some difficulty. The table gives three instances of this sort. The average of the tone sung in imitation of the standard is given first, followed by a number of measurements at the beginning of the tone sung in imitation of the distracting tone. The vagueness of the impression left by the latter tone is paralleled with the way in which the subject 'feels' for the tone when beginning it. After a few tenths of a second of uncertainty, however, he proceeds in the usual manner and with a fair approximation to the tone, which he is endeavoring to imitate.

TABLE XXXII.

SUBJECT FN.

Standard E = 160 Dis. F = 178.1	Standard C = 126.6 Dis. C♯ = 271.8	Standard D = 144.2 Dis. G = 186
E	C	D
162.9 (Av)	135.4 (Av)	145.0 (Av)
F	C♯	G
152.7	244.7	221
144.2	268.2	142.4
153	256.2	149.4
152.1	267.5	209.9
154.3	269.7	177.5
154.3	272.4	181.1
153.4	271.3	191.2
152	273.8	205
		197.5
		197.2
		197.5
		194.8

The most important results of the entire investigation may be summarized as follows:

I. In the singing of a tone a sudden marked rise in pitch usually occurs near the beginning of the tone. This rise in pitch is so general as to seem to indicate a universal tendency.

II. No tone is sung entirely uniformly. It oscillates in

pitch from period to period throughout its length in a somewhat irregular rhythmical fashion.

III. Very marked differences exist in different individuals with regard to their ability to imitate a standard tone. The subjects tested varied in the degree of accuracy in imitation of standard tones of different pitch from a small fraction of one per cent. to thirteen per cent. of error.

IV. There is manifest throughout a tendency to sing a tone higher than it should be sung. Thus the end of a tone is usually higher than the beginning and the sung tone is almost invariably higher than the standard tone.

V. Distractions when causing disturbances may affect the whole of the sung tone or only the beginning of the tone. In either case the effect of the distraction may be to cause the sung tone to vary from the standard (1) in the direction of the distracting tone; or (2) in the opposite direction from the distracting tone.

VI. Sung tones varying from a standard under the effect of distractions are usually harmonious with the distracting tones. When the distracting tone is inharmonious with the standard tone, distraction is more likely to occur than when the two tones form a harmony.

VII. A person may more or less successfully imitate a tone which he has heard when his attention was engrossed in singing another tone of a standard pitch.

VII. Conclusion.

The results of the investigations presented in the foregoing sections naturally divide themselves into three parts, viz.:

1. The singing of a tone when the effort of the singer is confined wholly to maintaining its uniformity;

2. The singing of a tone in imitation of a standard tone; and

3. The singing of a tone in imitation of a standard when distracting conditions are introduced by means of the sounding of other tones.

The explanation of the results requires a discussion of the relation between the type of reactions with which the investi-

gation deals and the nervous processes which lead to these reactions, and also a discussion of the various modifications which may enter into these nervous processes and consequently into the final form of reaction.

Even in the case of the simple tones, when the entire task of the subject is the uniform sustaining of the tone after it is begun, marked variations are to be found in the character of the muscular activity. These variations are of interest in throwing light upon the nature of the motor discharge which is their immediate cause.

As has already been pointed out in the course of the report, many of these muscular changes are not recognized by the subject. It does not follow from this fact, however, that the muscular changes have no definite relation to modifications in conscious processes. In order that the subject should be able to recognize these changes distinctly through introspection, it would be necessary not only that he should give attention to the sensory processes involved in the singing of the tone, but also that he should be able to give a certain surplus attention to the variations in his own reactions. The absence of introspective recognition of the variations, therefore, does not show that the process is a purely physiological process without relation to consciousness. It signifies merely an absence of perceptual recognition of the variations.

Even from the physiological point of view, the voluntary contraction of the muscles, such as would be called into play in the singing of any tone under even as simple conditions as those of the first series of experiments, is not as simple as might at first thought be supposed. The contraction is not uniform and therefore not due to a steady discharge of nervous energy into the muscles. It would appear rather that the nervous impulse is not continuous, but that it consists of a series of discharges rapidly following one another, the muscles responding by contraction to each discharge.

Investigators such as Helmholz have noticed that a muscle in the condition of contraction emits a sound of low pitch (about 38 or 36 vibrations per second). It was at first thought that this fact indicated the frequency of neural dis-

charge to the muscles, but Helmholz showed conclusively that the muscle sound cannot be relied upon for conclusions in respect to this rate. Helmholz himself set this rate at 19.5 per second. Various investigators have arrived at different conclusions in respect to this rate. Thus, Krönecker and Hall agree with Helmholz, but Schäfer concluded from his experiments that the period was one tenth second. Again Haycroft confirmed the time given by Helmholz as the period of the muscle tetanus, but found that this time is only a rough average of the frequency, the contractions themselves not being actually rhythmical. Haycroft's general conclusion is that "during a reflex or a voluntary movement, the muscles involved exhibit fascicular or other local movements due to uncoordinated discharge from the central nervous system and perhaps due also to variations in excitability or activity of the fibers or fasciculi affected."

These figures borrowed from the physiologists make it clear that muscular contractions, even in the voluntary movements of a simple kind, are subject to modifications which are the result of changes in the nervous condition. Reaction experiments give results which point to the same conclusion. Attention has already been called in earlier paragraphs to the fact that these irregularities in the vocal muscles are analogous to irregularities in the movement of the eye muscles and the muscles of the hand. Our own results argue against anything like an absolute rhythm of muscular contraction in connection with the vocal muscles. A period of 100 sigmas for making each measurement of the pitch brings out a more or less regular oscillation of the pitch. The measurements taken of tones for smaller periods show, however, that the rate of discharge is not constant even from moment to moment during the same tone.

The whole process of reaction and control may be described as one of constant nervous readjustment. All the factors of sensory excitation and motor response must be kept in constant equilibrium. This requires that there should be a continuous condition of active adjustment on the part of the subject. If the muscles tend to relax on account of their physical condition

to the point where the tone becomes notably different from that which the subject began to sing there will be a general readjustment, so as to recover the original pitch. The separate consciousness of the relaxation of muscles and the necessity of bringing them back to the original tone will seldom occur in the case of the subject who is sufficiently trained to maintain the reaction without separate conscious efforts. His attention will be entirely absorbed in the maintenance of the process and not in the observation of the process. We shall have, therefore, in the reaction itself the immediate physical parallel of the activity of attention without break or interruption and without any secondary introspective activity superimposed upon the primary process of reaction.

There will be one point in the whole process where the necessity of controlling the reaction is usually greatest and where in consequence the attention will be most keenly divided. This point will be at the beginning of the tone and, as has been shown in the results of all of the investigations, this is the point where the most characteristic changes in reaction appear. To a certain extent there is undoubtedly a physiological difficulty connected with the beginning of a tone. The muscles must undergo a change from a state of complete or partial relaxation to a state of tension. The higher the tone to be sung, the greater will be this tension and consequently the greater the physiological difficulty involved.

But even more significant than this purely muscular fact is the necessity of a general nervous adjustment which will bring the outgoing motor discharges to the desired level for the maintenance of a steady reaction of the kind desired. The conscious experience which a person has when he begins to sing a tone is entirely different from that which he has when the tone has been once thoroughly established. Among the changes which must occur in the nervous condition at the beginning of a tone are certain changes on the sensory side, due to the fact that as the tone begins to sound there are new sensations received from the muscles of the throat and new sensations in the subject's ears. These new sensations, together with the initial neural excitement, constitute a general sensory

motive for the reaction, the discharge to the vocal muscles resulting in a kind of nervous equilibrium. If the sound which enters the subject's ear is unsatisfactory for any reason, the equilibrium may again be overcome, so as to satisfy the subject's voluntary desire for the modification of the tone, but even where there is no separate consciousness of the necessity of readjustment there will always be an effort to bring the voluntary reaction and the sensory factors, which have aroused it, to the point where there is no excessive stimulation and no thoroughly incongruous form of reaction.

A general discussion of the nervous conditions which accompany attention has been undertaken by MacDougall in a series of articles in *Mind*.[1] These articles expound a theory of the physiological factors of the attention process and are very suggestive for our present discussion. MacDougall sets forth his scheme in the following words: "My scheme extends to the cell bodies of the neurones and to all their processes the theory of similarity of function that is accepted by most, in fact by almost all, physiologists as true for their axis-cylinder processes; and it assigns to the intercellular substances which, lying between such terminations of fibrils of different neurones or between such terminations and the bodies of other neurones, constitute the most essential parts of the synapses (or junctions of neurones), all those specific changes which are the psychophysical processes proper, the immediate physiological correlates or determinants of psychical effects; and it regards them also as the principal seats of those resistances, varied and variable in degree, which determine the passage of the excitation process in this or that direction and confine it to relatively well defined and narrow paths among the labyrinth of innumerable paths possible to it in the absence of such limiting resistances."

MacDougall's theory assigns to the synapses lying between the point of sensory stimulation and motor discharge various degrees of resistance corresponding to the degree of organization of the path involved. Each synapsis has a certain normal threshold value, but this value also varies according to the

[1] *Mind*, 1902, Vol. XI., p. 316; 1903, Vol. XII., pp. 289, 473; 1906, Vol. XV., p. 329.

degree of excitement in the neurones of which it forms the connections. Activity of the neurones tends to lessen the resistance of the synapses, while certain drugs, fatigue, etc., have the opposite effect. The degree of consciousness, then, according to this theory, instead of being related to the openness of the motor channel, depends upon the resistance offered by the synapses at various points in the course of the path involved. It is accordingly almost the exact converse of the theory of Münsterberg.

If now we wish to represent what takes place during the singing of a tone under the general scheme already presented. it may be expressed as follows. The voluntary contraction of a muscle corresponds to a central excitation in the motor area of the brain, causing a discharge into an afferent nerve leading to the vocal muscles. Now under condition of excitement it is one of the functions of all neurones to produce nervous energy at a rapid rate. The beginning of the supply of such energy is ultimately dependent upon the stimulation of a sense organ, but the supply becomes reënforced by the activity of all the neurones subsequently excited.

The discharge of the neural excitement, above referred to, into the vocal muscles causes contraction of these muscles. The contraction of the muscles in turn stimulates the afferent neurones leading from these muscles and thus reënforces the amount of available energy. This excitement returns to the muscle by way of a subcortical center, thus establishing what James has called a 'motor circle.' The return to the starting point inaugurates a fresh movement, which returns as before to the muscle. In this way the contraction of the muscles is maintained. If we regard the rate of this motor discharge as roughly approximating ten per second, a corresponding period of maximal contraction and partial relaxation may be looked for in the muscle.

A possible explanation of the sudden rise in pitch at the very beginning of the sung tone is also suggested by the same fact. If the theory is correct that the amount of available nervous energy is thus reënforced by that supplied from excitement of the sensory neurones leading from the muscles, then

the amount of energy to be used in the contraction of the muscles must be increased the moment after the contraction begins. This energy will be relatively more effective in increasing the height of the tone at its very beginning.

Moreover, at the beginning of the singing of any tone, still another sensation (auditory) is added over and above those which have been present the moment before. The sound of the sung tone increases the amount of available nervous energy. The most natural path of discharge of this energy is then the motor channel, already open. Hence at the moment after the singing has begun the amount of nervous discharge into the vocal muscles is suddenly increased from these two sources, leading to greater tension and an increase in the pitch of the tone.

Turning now from the simple process of maintaining a tone to the more complex processes involved in the imitation of a tone and in singing it in the face of a distraction, we see in view of the foregoing discussion that the maintenance of the equilibrium between the sensory motives for action and the motor discharges leading to muscular contraction becomes a very much more complicated process. In the case of imitating a given tone, for instance, the sensory stimulus consists not merely in the general directions that have been given to the reactor to produce a tone, but also in the auditory sensations which are intended to govern the specific character of the reaction; viz., the pitch of the sung tone.

The establishment of an equilibrium for which this sensory stimulation is the motive will take place in a similar manner to that in which it occurs in connection with the simple reaction. The two cases do not differ essentially in their fundamental type. But owing to the new sensory factors involved we may except in the latter case more frequent readjustments in the established equilibrium and hence larger variations in the reactions. In these more complex cases, too, we may expect greater individual differences due to the training of the subjects, some subjects being prepared by their earlier experiences to follow the specific instructions, while others will be more or less unprepared to do more than they did when there was no

specific tone to imitate. The accuracy with which the sung tone corresponds to the pitch of the standard depends upon the degree to which organization has been carried on in each individual.

When in addition to the standard tone, to be imitated, a further stimulus in the form of a distracting tone is introduced, it is to be expected that some disturbance of the established equilibrium will take place. This disturbance, however, will not necessarily be shown in a modification of the pitch of the sung tone. An equilibrium which is well established may maintain itself in the face of newly given stimulations if provision has been made for just such an emergency. This will often be the case under the conditions of laboratory experiments when the subject understands the significance of the distracting tone and when there is no vital or practical interest for the subject in the fact of its appearance. It is conceivable that, when a muscular coordination has once been established that the additional stimulus may simply reinforce certain of the motor discharges without leading to any modification in the form of that part of the reaction which relates to the pitch of the tone. In such cases the sounding of the distracting tone may necessitate a new equilibrium, but the features of the old equilibrium essential to its pitch are included in the new equilibrium as its most essential feature. The excessive stimulation from the added stimulus becomes a sensory motive for a discharge not into those motor channels connected with the vocal muscles, but into some other channels such as, for instance, those muscles controlling the volume of the tone, or it is drained off in the way of a more general motor excitement in various parts of the body. Other cases may occur as in connection with the series when a sudden loud noise was the form of the distraction, when the excessive stimulation thus aroused results in a temporary reinforcement of the motor discharges into the vocal muscles, but the pitch of the tone is not permanently affected.

If, however, the subject gives sufficient attention to the distracting stimulus to affect the pitch of the tone, it does not necessarily follow that the reaction will exhibit a modification

of the final vocal tone in the direction of the distraction itself, for in the effort to bring about a readjustment which shall include the distracting tone, the reactor may resist the tendency towards a new tone with such completeness as to modify his reaction in a direction exactly opposite to that of the new stimulus. This, as we have seen, occurs in a number of cases in the experiments reported. Analogous experiences occur in ordinary life. Instead of looking toward a bright light which distracts the subject, it is not at all unusual to turn away from this source of distraction. One may redouble his energy in reading if he is attempting to overcome the distraction of an external sound. The new stimulation is in both of these cases treated in such a way as to be eliminated from the equilibrium of attention rather than accepted as the guiding factor.

A fact of importance brought out in the results of the experiments is the tendency, when the distraction is effective, to sing tones which are harmonious with the distracting tone. It is by no means a new fact that the motor reactions to tones are intimately related to all the forms of discord and harmony. It may be advantageous in future discussions of the nature of harmony and discord to emphasize this relation to a still greater degree. There can be no doubt that the vocal cords will differ in behavior at different pitches in a manner analogous to the way in which different tones of the scale differ from one another in physical vibration and in their effects upon the basilar membrane.

Theories of harmony in the past have laid great stress upon the physical and sensory fact of vibration. A comparative study of musical statistics will show, however, that the development of the comprehension of musical harmony has taken place within a relatively short period. Furthermore, it is extremely unlikely that there have been modifications in the structure of the ear parallel to this development. It is not easy to believe that there is sufficient difference in the structure of the ear of the Chinaman and the American to account for the difference in their comprehension of musical harmony.

The results obtained from this investigation point to a line of consideration which it would be profitable to carry further.

The maintenance of an equilibrium which has been established between a sensory stimulation and a motor discharge is more difficult when an added sensory stimulation is of such a pitch that the motor discharges are inharmonious with the added stimulation. In other words, when the motor discharge is readjusted so as to establish an equilibrium that shall include the new stimulation and the earlier stimulations as well, there is an adherence to the principles which we know as the principles of harmony. It may, therefore, be said that there is a close relation between the recognition of harmonies and the motor processes and it is not necessary to believe that there is any necessary reference in this fact to the separate vibrations of the sensory stimulation or of the motor impulses. It does not seem necessary to assume that there is recognition of the tones from the separate vibrations.

The facts are explicable on the theory that the law of muscular adjustment must be in general conformity with the law of rates of vibration. This conformity is not the result of any conscious recognition of this relation, but is due to the fact that the muscular reaction, while it must on the one hand stand in some relation to the nervous conditions which control it, will naturally, on the other hand, be related to the facts of physical vibration, since the result of the muscular contraction is always physical vibration.

PRELIMINARY EXPERIMENTS ON WRITING REACTIONS.

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This paper reports a method of recording the movement made during the drawing of a line by means of a pencil as in writing. From this record the details of the form of the movement, the rate of the movement, and the changes in pressure which accompany the movement can be examined with great exactness. The measurements here reported are preliminary to an examination of different forms of writing and individual variations in writing movements. They give the time involved in starting simple and complex movements of different forms, and the time required to stop a vertical or circular movement when these are under way. In general it was found that stopping a movement involves more complex adjustments than starting, and when the final form of the movement is complex the time of starting is appreciably lengthened.

Apparatus.

The apparatus used in this experiment was devised for the purpose of making a general investigation of the writing coordination. The investigation has reached a stage which justifies a preliminary report giving a description of the apparatus, together with some account of experiments on the fundamental modes of reaction involved in writing, namely reaction by stopping, starting, and changing the direction of movements made with a pencil.

The method is essentially a modification of the kymograph method. A strip of paper, *B,* Fig. 76, travels across the writing surface, *A,* on which the reactions are made. As a moving strip is not a convenient surface upon which to write, a typewriter ribbon is placed above the moving strip and above the ribbon is a fixed sheet of paper on which the reactor writes. Thus, when the subject writes on the fixed sheet, the moving strip underneath takes the record from the ribbon as though the strip were being written upon directly, and at the same time the subject has the record of what he is writing before him as usual and is in no way discommoded by the movement of the

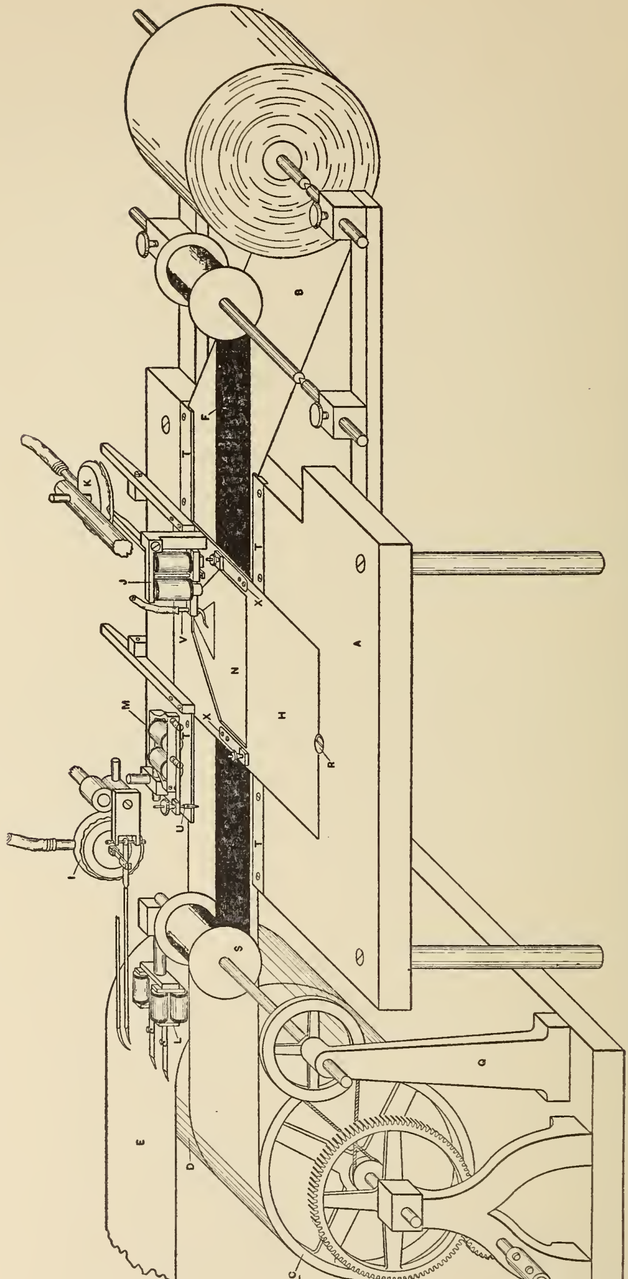

FIG. 76. (Reduced to one-sixth.) General view of apparatus. For detailed description see text.

strip underneath. The record on the upper sheet we may call the primary record and the record on the moving strip the secondary record. The sheet of paper and the strip, on which the primary and secondary records respectively are taken, will be referred to as the primary sheet and the moving strip. A sheet of carbon paper was first used for the transfer of the record, but it was found that the carbon soon wore off, especially when the pencil was held in one place for any length of time, and the ribbon moving at a slow rate was substituted.

Fig. 76 gives a general view of the apparatus. Two bars extending from the metal base *A* support the roll of paper and the spool of ribbon from which the strips *B* and *F* are unrolled. *B* and *F* pass across plate *A* to the drum *C* and the spool *S*, the latter being supported by the post *Q* and one not shown in the figure. The drum and spool are driven through spur gear connections by the shaft *G*, which is in turn connected with a driving shaft. The apparatus is coupled into the driving shaft and uncoupled by a friction clutch of the type shown in Fig. 97, page 371, of this volume. The apparatus can thus be set in motion at full speed, and it can also be instantly set free when the record is complete. The motion thus obtained is very regular and easy to control. The motion of the typewriter ribbon spool is greatly reduced from that of the drum, with the shaft of which it is connected by a belt.

In order to hold in position the primary sheet of paper on which the reactor writes and to support the hand, a plate, *H*, is placed over the primary sheet. A rectangular opening, *N*, is made in this plate to expose a writing surface on the primary sheet. The plate, *H*, is hinged at the back of the main base by two bars, so that it may be raised to insert the paper. Fig. 77 shows it raised from the base. Two small pins, *O*, *O*, pierce the primary sheet of paper and fit in the hole *R*, and one not shown in the figure, and keep the primary sheet from slipping when the strip and ribbon pass beneath it. These pins are above and below the moving strip. In order to get an even writing surface the plate is set into the main base, so as to lie flush with the general surface, and is held down by a screw, *R*, Fig. 76. The moving strip of paper and ribbon are also

set below the surface in a channel which is cut in the main base. Two guides, *T, T,* on each side of the moving strip keep it straight. The upper ones are slightly adjustable, so as to suit minor differences in the width of the paper.

In order to obtain a record of the relative position of the primary sheet and the moving strip, two pencil points are set through holes in the hinged plate, *H.* These pencil points make two dots upon the primary sheet and two lines on the strip. The points are shown in Fig. 76, *X, X,* and the holes through which they project in Fig. 77, *E, E.* The points are set on two flat springs and are adjustable with screws, so that they may be set against the paper with varying degrees of pressure.

Since the speed of movement of the strip is not perfectly uniform, an electric marker writing tenths of seconds, *J,* Fig. 76, is pivoted to a post set on the hinged plate, *H,* and is adjusted by a screw so as to bring the writing point against the paper. It writes through an opening in the primary sheet upon the moving strip beneath. In order to keep the primary sheet from blotting this line, it is held up from the moving strip by two small brass clips, *T,* Fig. 77, and the time line passes between these clips.

The glass pen, *V,* Fig. 76, which is used for the time record, is a form of capillary pen. To prevent clogging and uneven flowing, the opening in the point is made fairly large and the flow of ink controlled by a regulating air chamber. The upper end of the glass tube is inserted in a rubber tube which allows the point to move freely, and the tube is connected with a tambour. The rubber head of this tambour can be raised or lowered by a screw, and the ink thus made to flow slower or faster.

In order that the relation of the reactor's record to the time line might be easily determined, the reactor was required in this experiment to make all movements holding the pencil against guides which were placed in a known relation to the time marker. These guides can be set in the opening, *N.* One is a straight strip of brass which is a guide for the vertical line and the other a circular piece of brass which is a guide for the circle. The circular guide is about 4.5 cm. in diameter.

In order to get a record of the time when the signal was given in the reaction experiment, a second marker, *M*, Fig. 76, was clamped to the writing base and made to write on the moving strip. This marker is 14.5 cm. from the first marker, *J*, and by making a correction of this distance in reading the record, it can be correlated with the time marker and the traced record. This marker writes with a special form of pen.

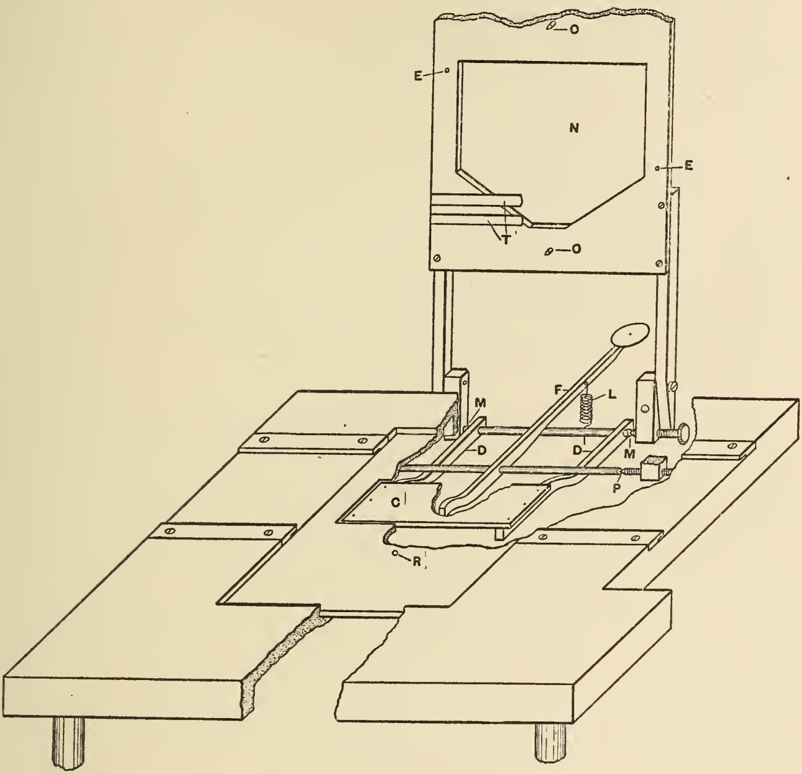

FIG. 77. (Reduced to one-fifth.) Supplementary view of apparatus.

This pen, *U*, Fig. 76, is made from a conical piece of steel by drilling a hole through the base and cutting a slit from this hole to the point. The marker is in circuit with a telegraph sounder which gives the stimulus for the reaction. Both marker and sounder are operated by a mercury key, thus giving on the moving strip with the other records the time of occurrence of the sound to which the subject is to react.

Besides the speed of the reactor's movements, it is desirable that the variations in the pressure of the pencil against the paper should also be recorded. The arrangement for securing a record of the pressure is shown in Fig. 77. Under the paper upon which the reactor writes is a small table *C*, set into an opening in the base. The opening in which this table is set is situated immediately below the opening, *N*, of the hinged plate, so that the table occupies all of the writing space. The table is capable of an upward and downward movement, for it is fixed to the two bars, *D, D*, which are in turn fixed to the axis working in the pivot joints, *M, M*. The radius of movement of the table is, accordingly, the length of the bars *D, D*, or 17 cm., and the direction of movement during a slight displacement is practically in a vertical line. The extent of movement of the table is magnified five times by means of the lever, *F*, which has its fulcrum at *P*. A disk on the outer end of this lever is in contact with the rubber of the tambour *K*, Fig. 76. The inner end of the lever, which is rounded, bears up against the table, making a sliding contact. In order to lessen the weight and consequent inertia of these parts, the table and its connections are made of aluminum. The long arm of the lever nearly balances the weight of the short arm together with the table and its supporting bars. The slight residue is counterbalanced by a light spring, *L*. This can be adjusted so that it will bring the table quickly back to position, but will not prevent a delicate response of the lever to a very light pressure on the table. The spring, *L*, as well as the tambour, with which a disk on the end of the lever is in light contact, are supported by a rod, shown in Fig. 76, fastened to the main base. The tambour is adjustable so that its head will just touch the lever when the table is in position. This apparatus responds with delicacy sufficient to easily record all the ordinary changes in pressure during writing. Tests with weights show that it will record changes in pressure of from 20 to 300 grams.

The remainder of the apparatus for recording pressure is shown in Fig. 76. The receiving tambour, *K*, is connected with the recording tambour *I*, which writes on a long strip of smoked paper, *E*. This strip travels over the drum, *D*, and another

drum 3.5 meters away. The drum *D* is clamped by an adjustable screw to the same shaft as the drum *C*, so that both of them can be driven together, or either one can be run separately by loosening the screw which clamps *D* to the shaft. Above the tambour pointer is a fixed pointer which traces a straight line with which to compare the pressure curve. The pressure curve is correlated with the speed curve on the moving strip, *B*, by means of one pointer of the double marker, *L*, which is in circuit with the marker, *J*, on strip *B*. The time of the reaction signal is also recorded on this smoked record by the other pointer of the marker, *L*, in the same circuit with the sounder.

To sum up then, by means of the apparatus above described, one can obtain two records, one showing all the details of the rate, and the other all the details of pressure in any writing or drawing movement. The following description will show the method of interpretation of the records secured with this apparatus.

Method.

Fig. 78 shows the simplest type of a record of speed. This is a record made on the moving strip of paper by drawing a vertical line on the primary sheet directly below the time marker. In order to make this record as simple and exact as possible the vertical guide was put on the hinged plate and the reactor was required to follow the guide in drawing this line.

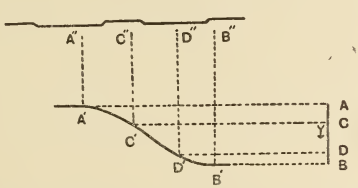

Fig. 78. (Reduced to one-third.) Illustration of the method of measuring the speed of a vertical line or part of a vertical line which is directly under the time marker. For detailed explanation see text.

The line *AB* represents the record on the primary sheet and *A′B′* and *A″B″* the record on the moving strip, *A′B′* being the record from the typewriter ribbon, *A″B″* the time line. All the dotted lines in Fig. 78 are inserted after the record to aid

in interpreting the full drawn lines which constitute the record proper. In order to measure the time occupied in drawing the whole line *AB,* we merely have to compare the whole tracing, *A′B′,* with the corresponding part of the time line, *A″B″.* The intervals of the time line indicate tenths of a second, and the drawing obviously occupied in this case about $1\frac{1}{4}$ tenths of a second or 125 σ. If now, instead of measuring the time of the whole line we wish to find the time consumed in drawing any given part of the line shown on the primary sheet of paper, *CD,* Fig. 78, we need only to project that portion of the vertical line to be measured upon the record taken on the moving strip. The projection of the vertical line upon the oblique record requires that the vertical line and the record be brought into the relation in which they were when the record was made. In order to do this, the reference points on the sheet made by

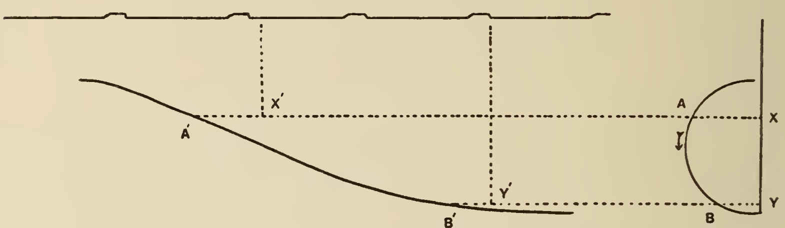

FIG. 79. (Reduced to one-third.) Illustration of the method of measuring the speed of part of a curve. For detailed explanation see text.

the pencils mentioned in describing the apparatus, are superimposed on the reference lines of the strip. Horizontal lines are now drawn from the beginning, *C,* and end, *D,* of the part of the vertical line to be measured, to cut the oblique line of the strip at *C′D′.* From this stage on the process is the same as in the first case.

The above is the simplest possible case. Very often the line is not drawn directly under the time marker. In such cases a correction must be made, in reading the record on the strip, equal to the horizontal distance between the marker and the line on the primary sheet. In this case the projection of the primary line, *AB,* Fig. 79, upon the secondary line, *A′ B′,* is found as before. Then the horizontal distance from the

primary line to the marker, AX, is measured, and the same distance taken from the traced line, $A'X'$. This brings the record of speed under that part of the time line which was made at the same time as the part of the line to be measured. Similar correction is made at B' by deducting a distance on the time line equal to BY. The time is now measured on the time line between points vertically above X' and Y'.

This figure shows the principle of correction when it becomes necessary to measure the time of any horizontal or oblique movement. Evidently in all such cases the distance between the two points on the secondary record on the moving strip represents not merely the distance through which the strip travels while the line is being drawn, but also the negative or positive change in horizontal position of the pencil point as it makes the line. The change in position of the pencil point must, therefore, be allowed for either negatively or positively in order to get the simple time determination. In order to make the necessary correction in reading the record, if the pencil moved in the same direction as the strip, this primary pencil movement must be added to the distance between the two corresponding points on the strip; if the pencil moved in the opposite direction, the distance must be subtracted. In the case of an oblique line the horizontal distance through which the pencil traveled must be allowed for, as in the case of the horizontal line. In projecting A and B in Fig. 79 to A' and B', the distances $A'X'$ and $B'Y'$ were recognized as unequal. The greater length of $A'X'$ shows that the pencil moved obliquely to the right as the line AB was drawn. Hence, a part of the record $A'B'$ is due not to the movement of the traveling strip, but to the movement of the pencil.

A typical record showing details of a simple reaction is presented in Fig. 80. S is the record of the reaction signal which is made by the second marker, M, Fig. 76, writing on the moving strip, B. In order to bring this signal into relation with the time line a correction has to be made equal to the distance between the two markers. Measuring off this distance on the time line, we find that the signal was given at a point on the time line corresponding to S'. The point at

which the reaction occurred is indicated at R. The point where the signal is given, thus corrected, will hereafter in all cases be indicated by the letter S, and the point of the reaction by R.

The error in reading the records was well within three thousandths of a second. Each interval of the marker record, that is, each 100 σ was in the records 30 mm. or more in length. Accordingly, a distance corresponding to one sigma was at

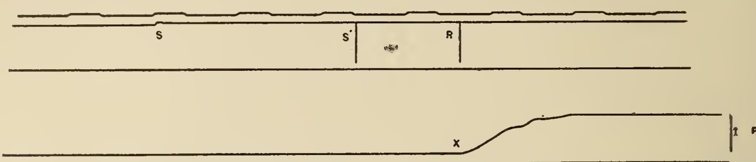

FIG. 80. (Reduced to one-third.) Speed curve from a reaction by starting a vertical movement upward, showing at P the line as actually drawn, in the upper line the time record, at S the record of the signal for reaction, and S' the corrected position of S with reference to the time line. R corresponds to the point X where the reaction began. The reaction, measured by the part of the time line between S' and R, in this case was very fast, occupying about 125σ.

least one third of a millimeter in length. This could be read with great precision, the measuring apparatus reading directly to tenths of a millimeter. Allowing for all possible errors up to a full millimeter, the error of measurement would fall well within three sigmas.

The reactions reported in this paper consisted of various fundamental movements which occur in all complex writing activities. The reactor held a lead pencil in his hand in the ordinary position for writing and drew lines of various kinds across the primary sheet of paper. The results of this experiment, therefore, constitute an introduction to the study of writing. They also have independent value as showing certain characteristics of various types of reaction movements. In ordinary reaction experiments the movement which constitutes the reaction is very simple, like lifting the finger from the key, and very little is known of the subsequent or antecedent motor changes. Even these simple reactions may very advantageously be subjected to analysis, as has been shown by a number of recent investigations. The present investigation serves to describe a method of complete analysis and to supple-

ment the analytical work already done on simple reactions by an analysis of more complex reactions. In the second place the analysis was made complete in two directions in that it includes both horizontal movements and pressure changes. Pressure changes may precede, accompany, or follow the horizontal movement in which the main reaction consists. The secondary phases of movement often throw light on the character of the whole process, as subsequent discussions will make clear.

In the third place, we may by this method study reactions of a type which are ordinarily neglected in reaction experiments, namely, those which consist in stopping a movement. Previous experiments have usually dealt altogether with reactions by starting a movement. We may refer to those common movements as starting reactions. In distinction we may designate reactions by stopping a movement as stopping reactions. In ordinary behavior stopping reactions are as constant and as important as starting reactions. In fact, most reactions are a combination of the two. This is particularly the case with the reactions which occur in writing, as will be seen when one considers a case in which the direction of a line is changed abruptly.

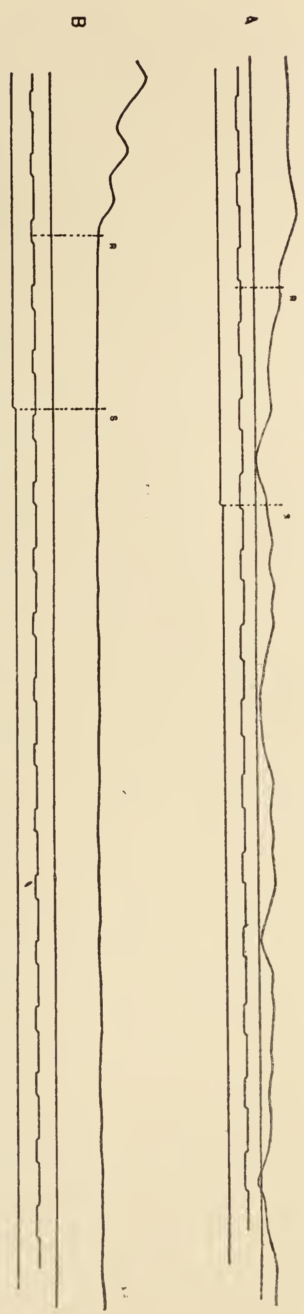

FIG. 81. (Reduced to one-fourth.) Illustration of two typical records of pressure. *A* is from a reaction by stopping a vertical movement and *B* from a reaction by starting a vertical movement.

Since starting reactions are of the more commonly investigated type, we present first a typical case of such reaction. A specimen of the speed record for a starting reaction is shown in Fig. 80, and the pressure record for the same movement is shown in Fig. 81, *B*. In the speed record, Fig. 80, we have the primary line, *P*, and the secondary curve, *X*. The curve consists merely of a straight line before the reaction begins—that is, on the left of *X*. This line is made by holding the pencil at a point, preparatory to the reaction. At the point, *X*, where the line on the record deviates from a straight line, the reaction begins and we may calculate the reaction time by comparing the distance from this point to the point which records the giving of the signal to react, with the time line. From the record after the reaction we can calculate the speed of the post-reaction movement. The more abrupt the deviation of the line from the horizontal, the faster was the movement. Other characteristics come out in the form of the record. In the record before us the movement exhibits marked irregularity in speed, there being two parts of the movement which are much slower than the rest, as shown by the wave form of the line at the right of *X*.

Turning to the pressure curve in Fig. 81, *B, S* is the point at which the signal was given and *R* the point where the reaction occurred, the record in this case being read from right to left. On the right of *S* some slight irregularities are seen in the pressure line. These are less obvious in the reduced figure than in the original record. These irregularities show changes in tension prior to the reaction. Similar facts were reported at length in Vol. I. (New Series) of these Studies (pages 141–184). In this experiment there are shown in addition to the variations in pressure before the reaction, those which occur after it.

The speed and pressure records for a stopping reaction are presented in Figs. 82 and 81, *A*. The first part of the speed line, Fig. 82, shows that a vertical movement is being made. At the right end of the record the oblique line changes into a horizontal; this shows the inhibition of the movement. The time which elapsed between the signal to stop and the actual

stopping can be measured as before. It is especially important to observe that the movement continued for some time after the signal, but is, especially at the end, modified in form. The pressure record, Fig. 81, *A,* which should be read from right to left, exhibits the changes in pressure which occur during a vertical movement, and the increase in the downward pressure after the vertical movement has entirely stopped. *S* and *R,* as before, indicate the giving of the signal and the reaction.

The experiments to be reported fall into seven groups. The details regarding these seven groups are as follows. In Series I. the reaction consisted in stopping a circular movement. The circular attachment was put in the opening of the plate *H,* Fig. 76. The reactor was required to move in an anti-clockwise

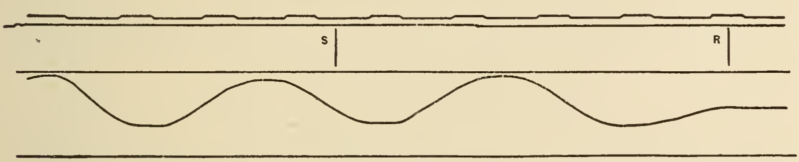

FIG. 82. (Reduced to one-third.) Speed curve from a reaction by stopping a vertical movement, showing in the uppermost line the time record, and in the second line from the top at the extreme left the signal to react. This is corrected to *S*. The line shows at the right of *S* the break in the circuit of the signal marker, which is not, however, significant. The third line from the top and the bottom line are reference lines. The line between the reference lines shows at the left a succession of upward and downward movements and at the right the stopping of these movements.

direction, keeping the point of the pencil against the inner edge of the guide. The movement was made at a rate of speed which was agreeable to the reactor, and varied from one to two revolutions a second. The reaction consisted in stopping the movement as abruptly as possible when the sound was heard.

In Series II. the reaction consisted in starting the circular movement which was used in Series I. The pencil was held at a point against the inner edge of the circular guide and at the signal the reactor began to trace a complete circle. In one half the reactions the point at which the movement began was at the extreme left side of the circle, and in the other half at the extreme right side.

In Series III. the reaction consisted in stopping a vertical movement. The vertical guide was put in the opening of the hinged plate. The reactor moved the pencil up and down against this vertical guide, tracing a line on the paper. The movements were made at the rate of about two double strokes per second and were from two to three centimeters in extent.

In Series IV. the reaction consisted in starting the movement described in Series III. The reactor held the pencil at a fixed point against the straight edge and at the signal made a single stroke. In half the cases the movement was made from the top down, and in the other half from the bottom up.

In Series V. the same vertical movement as in Series III. was made before the reaction. The reaction itself, however, consisted in changing the direction of the movement from the

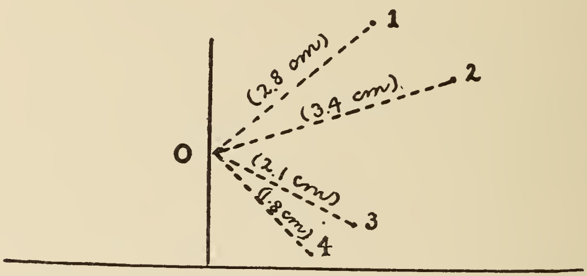

FIG. 83. (Natural size.) Position of the points used in the reactions in series VI which consisted in moving to points. *O* is the point on the straight-edge at which the pencil was held preparatory to the movement in each reaction.

vertical to the horizontal, instead of merely stopping it. At the signal the reactor moved the pencil from the vertical, whatever its position happened to be, through a convenient distance horizontally to the right.

In Series VI. the reaction consisted in moving from a fixed point, *O,* Fig. 83, which was directly under the time marker, to a dot which was placed on the paper. Four dots were used for successive trials in the positions and at the distances indicated in Fig. 83. The reactor was instructed to move to the point with one stroke.

In Series VII. the reaction consisted in starting to make a square or a circle. The pencil was held before the reaction

directly under the marker, as in Series VI. The square or circle as made are shown in Fig. 84. In half the cases the reactor started up and in the other half down.

In most of the reactions of all the series a warning signal

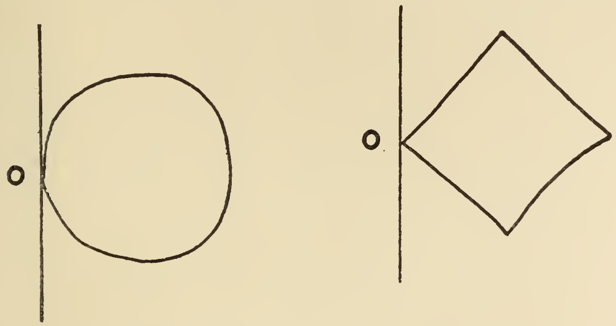

FIG. 84. (Natural size.) Types of the figures made in the reactions by starting to make geometrical figures.

was given by saying 'ready' from one to two seconds before the signal for reaction.

There were five subjects in the experiment, all of whom were trained in reaction work. They were Chas. H. Judd, E. H. Cameron, D. J. Cowling, C. A. Cockayne and the writer. They will hereafter be referred to by their initials.

RESULTS.

The results of the experiment fall under three general heads. First, the reaction times of the various series will be reported in thousandths of a second. Second, a description will be given of the form of the speed curve and the rate of its different parts. Third, a report will be given of the pressure changes which occur before the signal, between the signal and the reaction, and after the reaction.

The reaction times obtained in each of the series of the experiment are shown in Table I. This table gives the number of reactions, the average reaction time, and the mean variations for each reactor in each series. The total number of reactions and general average is also given for each series.

The mean variations are in some cases within ten per cent. of the average, but are usually larger than ten per cent. and in

TABLE I.

Reaction Times of the Different Subjects under the Various Conditions.

Subject.	I. Stopping Circle.			II. Starting Circle.			III. Stopping Vertical Movement.			IV. Starting Vertical Movement.		
	No. reac.	Avg.	M.V.	No.	Avg.	M.V.	No.	Avg.	M.V.	No.	Avg.	M.V
C. H. J.	14	244	43	17	213	31	15	243	48	16	196	43
E. H. C.	12	239	58	15	171	18	10	256	69	10	186	25
C. A. C.							7	351	53	8	301	57
F. N. F.	9	253	20	18	187	40	9	306	43	12	175	16
D. J. C.	11	385	69	11	170	32	7	226	40	10	160	22
Avg.	46	278		61	187		48	271		56	198	

Subject.	V. Changing Direction of Movement.			VI. Starting to Points.			VII. Starting to make Geometrical Figure.		
	No.	Avg.	M.V.	No.	Avg.	M V.	No.	Avg.	M V.
C. H. J.	10	247	28	6	209	44	8	263	55
E. H. C.	10	312	47	11	233	26	11	223	38
C. A. C.	4	338	11	8	273	50	9	324	67
F. N. F.				8	230	23			
D. J. C.	10	403	71	10	198	34	9	183	30
Avg.	34	323		43	228		37	247	

several cases are about twenty per cent. The explanation of this large mean variation is probably to be found in the complexity of conditions which were imposed on the reactors.

A comparison of the stopping reactions with the starting reactions shows that the stopping reactions are markedly slower. Take first Series III., stopping a vertical movement, and Series IV., starting a vertical movement. The average of the stopping reactions is longer by 73 σ or 37 per cent. than the average of the starting reactions, and the average for each subject shows a like difference. The same general result appears in the starting and stopping of circular movements. In dealing with the vertical movements, it was thought that possibly the pause and abrupt changes in direction in the vertical movements prior to the particular movement in which the stopping occurred, might be the cause of the delay in stopping when the signal was given, and the circle was accordingly introduced to secure a movement without pauses or abrupt changes in direction. By comparing the results of Series I. and II., it will be seen that there is a still greater difference between the time of stopping and starting a circular movement, viz., 91 σ or 49 per cent. The stopping reactions are slower than the starting reactions in this case also for each reactor.

The averages of the two other starting series, Series VI., starting to points, and Series VII., starting to make a geometrical figure, are also, in spite of the complexity of the conditions in these series, for the most part smaller than the averages of the stopping reactions. This rule holds in the case of the individual reactors with but one exception. For C. H. J., Series VII., starting to make geometrical figures was slower than the stopping reactions.

Turning to a comparison of all the starting reactions, viz., Series II., starting a circle; Series IV., starting a vertical movement; Series VI., starting to points; and Series VII., starting to make geometrical figures; it is seen that the last two series, which involve complex preparation, are noticeably longer in most cases than the first two. This result holds for the individual reactors with two exceptions; with C. H. J., starting to points is faster than starting a movement in a circle; and with C. A. C., starting to points is faster than starting a vertical movement. These are but two exceptions out of eighteen cases. It appears, then, that a reaction in which the succeeding movement has a greater number of determining conditions is slower even in starting than a reaction in which the movement has fewer conditions. In starting a movement to a point the reactor has not only to make a movement in a specified direction, as in starting a movement in a circle or along a straight edge, but he has also to stop the movement at a given point. In starting to make a geometrical figure, he has to move through a given distance and then change the direction of the movement. The later stages of the movement are evidently prepared for before the actual turn or pause is made, so that the earlier phases of the reaction are modified in view of the later requirements. Changing the direction of a movement, Series V., has the longest reaction time of all with two individual exceptions. For C. H. J., Series VII., starting to make geometrical figures, is longer, and for C. A. C., Series VII., stopping a vertical movement is longer. This reaction, as will be pointed out in the later analysis, is the most complex of all.

Turning from a consideration of the reaction times to the

character of the records, it will be found convenient to consider separately three parts of the records, namely: first, the parts before the signal for reaction; second, the parts between the signal and the reaction; and, third, the parts after the reaction.

The traced records before the signal are of interest only in the stopping reactions, viz., in Series I. and III., and in the

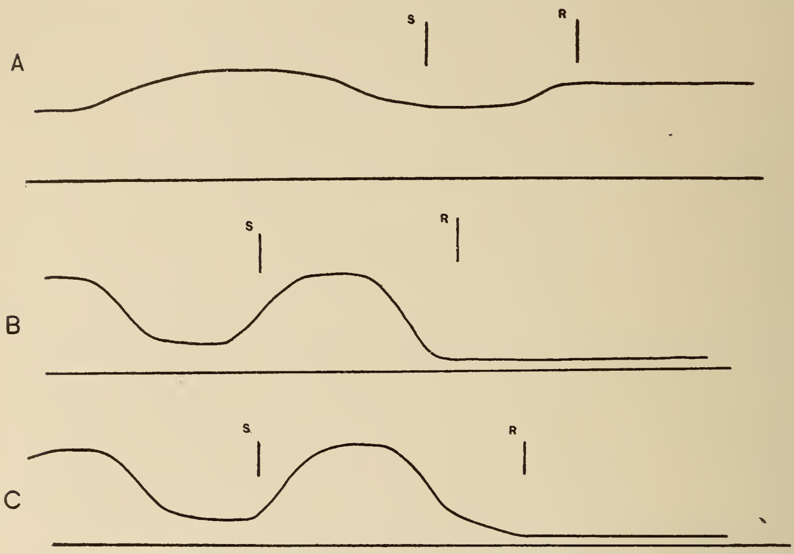

FIG. 85. (Reduced to one-third.) Speed curves from reactions by stopping a vertical movement. *S* indicates the point where the signal is given and *R* where the reaction occurs. *A* shows increase in speed of upward movement between the signal and reaction, *B* shows increase in the speed and amplitude of the movement and *C* shows decrease in the speed and increase in the amplitude.

records of changes in the direction of movement, in Series V. In the other series the traced record before the reaction is merely a straight line. Fig. 85 gives three records from Series III., and Fig. 86 gives five records from Series V. All of these curves are records which can be used in determining the speed of straight line movements. In all cases the speed variations before the signal to react are fairly regular. In the case of vertical movements the speed varies from a maximum at the middle of each stroke to a minimum toward each end, with a final pause at the end of each stroke.

Series I. gave records of circular movements prior to the reaction. In order to ascertain the relative speed of different parts of circular movements, measurements of time required for equal arcs of upward and downward circular movements were made. The results of these measurements are interesting on account of their bearing on writing. Table II. gives the average time of the upward and downward strokes, the difference between them and the number of cases in which the downward strokes and the upward strokes, respectively, were slower. It will be seen that although each person shows a rather uniform relation between the speed of the upward and downward strokes, no two of the subjects of the experiment belong to the same type. Measurements would have to be taken from a large number of subjects in order to determine what is the most common type and what are the causes of the difference between different individuals.

TABLE II.

COMPARISON OF UPWARD AND DOWNWARD STROKES IN MAKING CIRCLES.

Subject.	No. of Cases.	Average Downward Strokes. Sigmas.	Average Upward Strokes. Sigmas.	Difference.	No. of Cases in Which Downward Strokes Were Slower.	No. of Cases in Which Upward Strokes Were Slower.
C. H. J.	15	1,509	1,435	— 74	13	2
F. N. F.	16	1,407	1,395	— 12	11	5
D. J. C.	11	998	1,003	+ 5	5	6
E. H. C.	13	1,409	1,549	+140	1	12

No new observation regarding the changes of rate of movements before the reaction need to be added in view of the report given for Series III. and V.

Turning now to the second part of the traced record, that lying between the signal to react and the final movement of reaction, we are again confined to Series I., III. and V. A number of typical cases are presented in Figs. 85 and 86. In some cases the speed of the movement is increased, as in *A* and *B*, Fig. 85, and *D*, Fig. 86. The record line here falls or rises more abruptly than in the preceding part of the record. Conversely, a larger number of cases show that the rate of a preliminary movement is gradually reduced as the moment of reaction approaches, indicating that the usual method of stopping a movement was to 'slow down' gradually to a stop. This change is seen in *C*, Fig. 85, and *C*, Fig. 86. This would

seem to be the natural method of stopping. Where there is an abrupt acceleration of speed before reaction followed by a sudden stopping, we have a result analogous to the antagonistic reaction noted by Smith in *Mind,* Vol. XII., pp. 47–58, and in Vol. I. (New Series) of these Studies, pp. 141–184.

Changes in the amplitude of the movement can obviously not occur when the circular guide is used, but only with the straight edge, in stopping or changing the direction of a vertical movement, Series III. and V. Such changes between the signal and the reaction are shown in curves *B* and *C,* Fig. 85, and *C,* Fig. 86. Decrease in amplitude is shown by the fact that the upper part of the tracing is lower or the bottom higher than in the preceding part of the line, so that the vertical distance between them is less. Change in amplitude almost always consists in a decrease of the amplitude, indicating that in this respect, as in speed, the movement comes to a stop gradually.

In certain cases of vertical movement there is an increase in the length of the pause at the beginning of the last preliminary movement. There is always a pause, as was noted above, at the top and bottom of each vertical movement. When this pause is lengthened before the last preliminary movement it indicates clearly a conflict of tendencies. The movement is well established and there is a strong tendency to continue it. The signal calls for a stopping of the movement and is only slowly and partially successful in bringing about its result. The lengthening of these pauses between the signal and the reaction occurs chiefly in changing the direction of a movement, Series V., which is evidently a complex form of reaction, as shown by the time given in Table I. Such a lengthening is clearly shown in curves *A, B* and *D,* Fig. 86. This increase in time of the reaction and change in the form of the movement shows that what usually occurred was that the horizontal movement was not continuous with the vertical movement, but the vertical movement was first stopped and then the horizontal movement was begun. There are thirty-three reactions in which the above-mentioned changes in the character of the movement occur between the signal and the

reaction. In some reactions more than one change is present, as, for example, a decrease in the amplitude and a decrease in the speed of the movement.

Significant traced records after the reaction appear chiefly in Series V. The character of the primary line in Series V.,

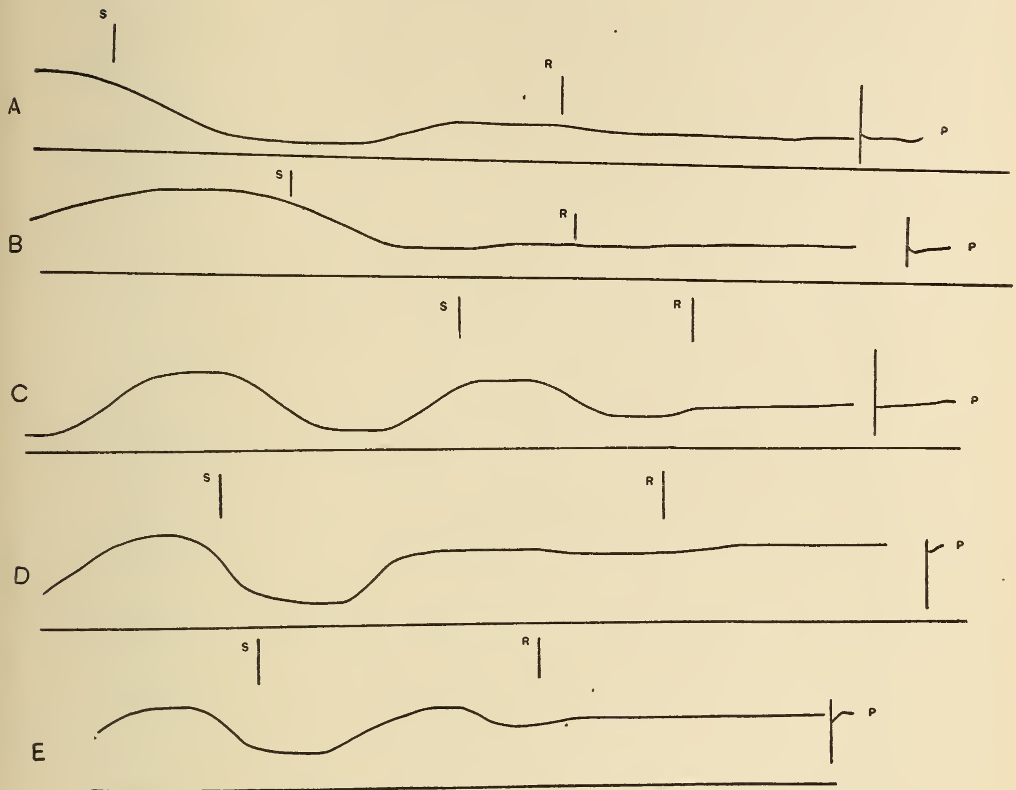

FIG. 86. (Reduced to one-third.) Speed curve from reactions which consisted in changing the direction of a movement, and the corresponding lines as actually drawn on the primary sheet. The latter are indicated by the letter *P, P* . . . . *A* shows a decrease in the speed of the vertical movement, a pause before the initiation of the horizontal movement and an inclination of the horizontal movement downward toward the movement just preceding it. *B* shows a pause before the reaction and an inclination of the horizontal movement away from the movement just preceding it. *C* shows a decrease in the speed and amplitude of the vertical movement before the reaction and an unusually straight horizontal primary line. *D* shows marked increase in speed before the reaction and a pronounced pause between the signal and the reaction. *E* shows an inclination of the horizontal movement toward the downward movement just preceding it.

which was made by changing the direction of a movement, is worthy of detailed examination. Examples of these lines are shown in Fig. 86 and are indicated by the letter *P*. These are the primary lines of which the long curves are the secondary lines. The general fact is that the apex of the angle which the horizontal line forms with the vertical line is very much rounded and the first part of the horizontal line is a pronounced curve. *A, B* and *E,* Fig. 86, are examples of two types of these lines. In *B* the concave side of the curve faces the last stroke before the horizontal movement. This is the more usual type. In *A* and *E* the curve is in the opposite direction. This form occurs in a number of cases. *C* is an unusually close approximation to a horizontal line. The straight horizontal movement, then, is not attained immediately, but there is a vertical component at first, which disappears gradually. The variations in form show how difficult it is to secure anything like a separate and wholly new movement immediately. Here the established vertical movement tends to pass over gradually into the new movement. In contrast with the earlier cases in which it was shown that preparation for a later movement reaches back into the early stages of a reaction, the influence here is forward. Though the two types of cases differ in the direction of influence, they agree in the general fact that there is a tendency for any single reaction to reflect in its character and rate the surrounding condition in which it occurs. The traced curves show also the transition to the new movement.

We come now to a consideration of the pressure records. A pressure record was taken for each subject, but as it was not convenient to prepare more than one strip of smoked paper for each period, only two or three pressure records on the average were taken with each series. Since the characteristics of the pressure curves were fairly uniform for each kind of reaction, however, and since they were used only for qualitative determinations, this number of records is sufficient as a basis for the description of pressure changes. There are 59 pressure records in all, distributed among the series as follows: Series I., stopping a movement in a circle, 6; Series II., starting a move-

ment in a circle, 7; Series III., stopping a vertical movement, 18; Series IV., starting a vertical movement, 7; Series V., changing the direction of a movement, 6; Series VI., starting to points, 6; Series VII., starting to make geometrical figures, 9.

Fig. 81 shows two typical records, *A* from D. J. C. taken from Series III., stopping a vertical movement; and *B* from C. A. C. taken from Series IV., starting a vertical movement down. The lower line in both records is from the signal marker and the break indicates the signal to react. The second line from the bottom is the time line marking tenths of seconds. The third line is the straight line traced as a base for comparison with the pressure curve. The fourth line is the pressure record. As *B* is the simpler record, we will first analyze that. There first appears before the signal a slightly wavy line, whose crests are from 50 σ to 100 σ apart. The pencil was here held at the fixed point before the reaction with considerable pressure and with slight oscillations in pressure, these oscillations being of a tenth of a second or less in duration. These waves can be satisfactorily explained as due to the succession of nervous impulses necessary to hold the hand in a fixed position. Between the signal and the reaction the waves are less prominent, indicating a change in tension preparatory to the reaction. Just before the reaction shown on the traced record, the pressure begins to increase gradually, and finally, soon after the reaction, which consists in beginning a movement along the straight edge, it increases rapidly by three stages. This marked rise in pressure after the reaction is a feature which appears in nearly all of the reactions. Indeed, the examination of such a record as this gives one a very striking example of the complexity of a reaction. The drawing of a vertical line is evidently accompanied by a series of pressures against the writing surface which must be important in getting the muscular mechanism and the nervous mechanism into operation, but all of which are neglected in any ordinary record of action.

Record *A*, Fig. 81, shows the typical variations in pressure during the making of a series of up and down vertical move-

ments. There are minor variations among different subjects, but the general characteristics for such movements are alike. For the same subject, as will be seen from this record, successive movements of the same kind are accompanied by practically the same pressure variations. In the present case a detailed comparison of the pressure record with the speed record shows that for a straight up and down movement the point of least pressure is about two thirds of the way on the upward stroke. The pressure then increases as the stroke approaches the top, continues to increase, sometimes after a slight decrease, as the stroke comes down and reaches its maximum intensity half to three quarters of the way on the downward stroke. It then decreases and reaches its lowest point again a little past the middle of the upward stroke. A modification of this series which frequently occurs consists of a small secondary increase in pressure at the beginning of each stroke.

The other significant facts shown in record *A*, Fig. 81, are the changes in the pressure variations between the signal and the reaction and the great increase in pressure after the reaction. The signal for reaction comes when the upward stroke is about one third completed. The pressure decreases as usual to its minimum as the stroke goes up, increases as the stroke approaches the top and after a slight decrease increases as the stroke goes down. The pressure changes are as usual up to this point. Here, however, instead of decreasing as the stroke reaches the bottom, it increases rapidly. It remains constant till a short time after the reaction and then increases to about double its previous greatest intensity. This pressure is then maintained. This shows a notable pressure reaction before the primary reaction.

A number of curves showing other examples of pressure changes with some variation are presented in Fig. 87. Record A, from C. A. C., is one of the several cases in which an effect of the warning signal in a change of pressure, without an actual premature reaction, is evident. The fall of the pressure curve below the base line near the right-hand end of the curve shows a partial reaction to the warning signal by a change of pressure, but there was no movement of the pencil across the

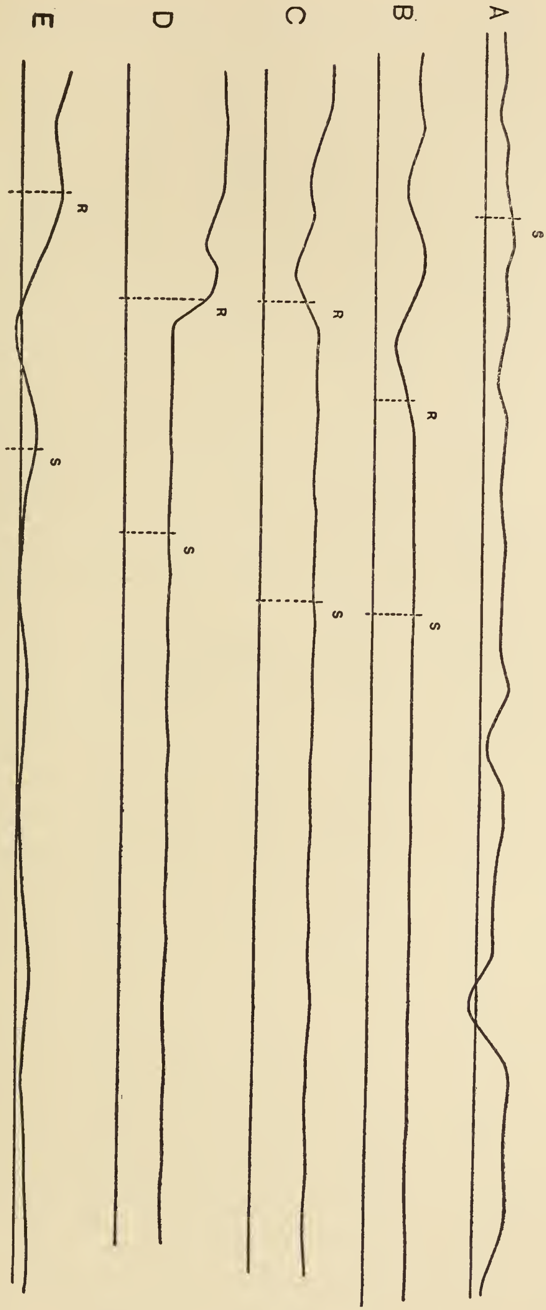

FIG. 87. (Reduced to one-third.) Records of pressure changes during reaction. *A*, no reaction; *B*, starting to points; *C*, *D*, starting to make a geometrical figure; *E*, stopping a vertical movement.

paper either here or after the reaction signal. In some cases there was also an actual premature reaction to warning, both of pressure and movement across the paper.

Curve *B*, from C. A. C., is from the pressure record of a reaction in Series VI., starting to points. This record shows a slight waviness of the pressure line before the reaction. About 35 σ before the true reaction there is a pronounced decrease in the pressure. After the reaction the pressure continues to decrease, then increases and oscillates while the reaction line is being drawn. Here again the pressure reaction and the reaction proper are seen to be different phases of the total reaction.

C is from E. H. C., in Series VII., starting to make a square. The waviness of the pressure line while the pencil is held at a fixed point before the reaction is here well marked, and continues up to the moment of the reaction. The pressure decreases before the reaction as in *B*, and increases somewhat more after the reaction.

A comparison of the average height of curves *B* and *C* indicates a typical difference in the amount of pressure exerted by different individuals. Pressure also varies somewhat with the same individual at different times.

D is from the same subject and series as *C*. It has the same characteristics until just before the reaction. In this reaction the pressure increases instead of diminishing at the reaction. The increase of pressure which accompanies the drawing of this line begins, as in the previous case, before the reaction. The intensity of pressure after the reaction is greater in this case.

E is from C. H. J., in Series III., stopping a vertical movement. It exhibits the typical pressure variations accompanying the vertical movements. The reaction comes at a point about two thirds down the stroke. The pressure ordinarily rises in this part of the record to its maximum intensity, but in this case the rise in the pressure is directly related to the reaction, as is shown by the fact that it is more than double that of any previous stroke. This is also another example of a pressure change coming before the reaction. After the reac-

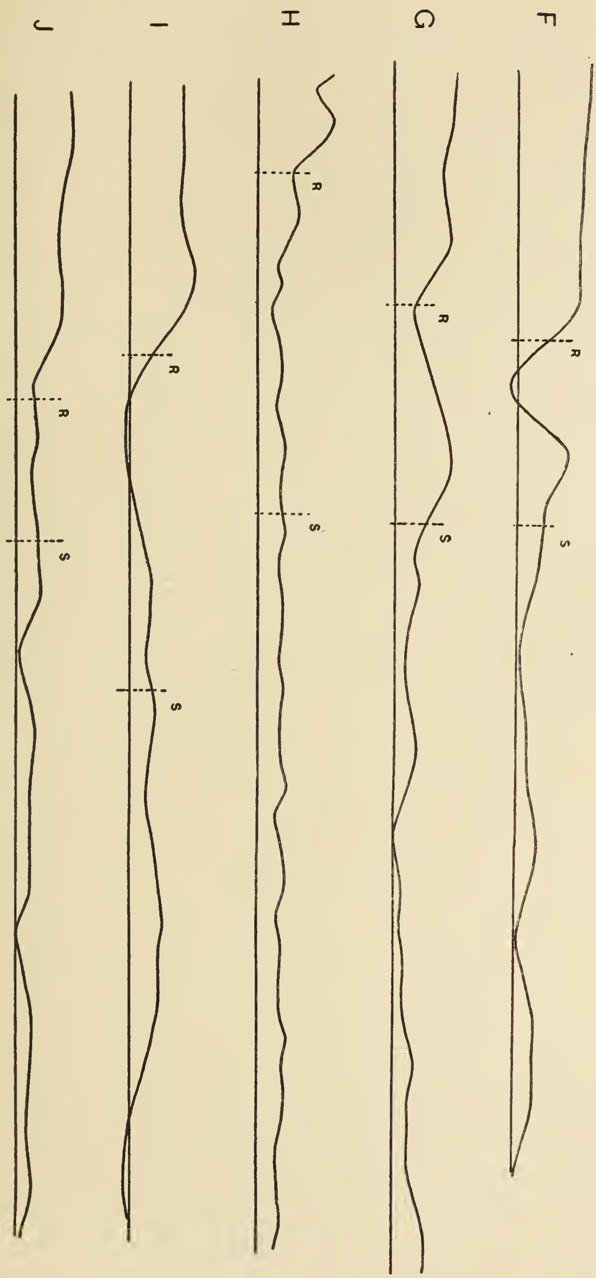

FIG. 88. (Reduced to one-third.) Records of pressure changes during reac tion. *F*, *G*, and *J*, stopping a vertical movement; *H*, changing direction of movement; *I*, stopping circular movement.

tion, the pressure decreases slightly and then increases again as usual. While the pencil is held at a fixed point after the reaction the pressure is not so steady as it is when the pencil is held at a fixed point under ordinary circumstances. It oscillates with a rhythm similar to the pressure variations which accompanied the movement before the reaction.

Fig. 13 shows similar pressure curves.

F, from the same reactor and series as *E*, is typical of certain cases in which a pressure change similar to those which are described above occur before the reaction comes and within 100 σ of the signal to react. The pressure reaction time is accordingly as short as the most rapid forms, while the apparent time measured from the primary movement is slower than the usual reaction.

G is another record from the same reactor and series showing the same sort of pressure change coming about 100 σ after the signal. These records show that there may be a response to a signal in less than 100 σ.

H is from C. A. C., Series V., changing the direction of a movement. This record shows especially regular pressure variations for the preparatory movement. This movement—the vertical movement—stops a little over half way up the stroke. The pressure here increases with a little jerk, producing the curve on the record about two thirds of the way from the signal to the reaction. The hand then pauses before beginning the horizontal movement which constitutes the reaction. Meanwhile the pressure increases rapidly until the reaction. This is another case in which the increase in pressure comes before the movement which it ordinarily accompanies and which constitutes the reaction. The movement which was intended as a horizontal movement is of the second type described above (*E*, Fig. 87), its convex side faces toward the movement last made. That is, it turns back on the last stroke. The pressure increases rapidly as this movement is made.

I is from F. N. F., Series I., stopping a circular movement. The extremely low pressure comes in the fourth quadrant of the circle. In this quadrant the pencil twice left the paper. The reaction occurs about half way on the upward movement,

that is, at the extreme right-hand side of the circle. The pressure increases as usual up to this point and when the pencil ceases to move at the reaction, the pressure continues to increase rapidly till it reaches a high level, which is maintained.

J, from C. H. J., in Series III., stopping a vertical movement, shows the usual rise in pressure after the reaction, and a continued oscillation, suggesting, as in the record *E,* the continuance of the pressure changes which accompany the previous movement.

The characteristics which have been pointed out in these records are typical of the pressure changes accompanying the reactions. For example, out of the twenty-nine records in which the pencil was held at a fixed point before the reaction, fifteen showed a waviness in the pressure curve. Of all the records in which a reaction took place, thirty showed a noticeable change in pressure between the signal and the reaction. In twenty-five cases the pressure increased and in five it decreased. In all cases there is marked change in pressure after the reaction, usually a very pronounced increase. In half of the cases in which a movement is followed by holding the pencil at a fixed point, that is, in eleven out of twenty-two, there is a noticeable oscillation in the pressure line while the pencil is thus held, suggesting a continuation of the previous pressure variations.

Discussion.

In the discussion of the results of this experiment it will be necessary to abandon the distinctions which, for the sake of clearness in description, have been drawn between the various phases of the reaction. No one of the elements, as for example the reaction time, can be considered alone. The difference in the time of various reactions must clearly include some reference to the complex of activity which makes up the reaction. To separate the duration from the other characteristics is to make an arbitrary distinction which obscures rather than aids in the study of the problem.

The point of departure for the explanatory discussion may be taken from the fact first noticed, namely, that the stopping

reactions were slower than the starting reactions. Before generalizing on the results it should perhaps be noted that neither the movements in a straight line nor in a circle, as arranged with the apparatus described, were continuous in a single direction. It is very desirable that measurements of stopping and starting reactions be taken with long movements in a single line. It may be said, however, that the forms of movement used in the experiment are more closely analogous to the great majority of the movements which one makes in every-day life than the simpler form would be. That is, most movements, as in walking, writing, or manipulation of any kind, and also the involuntary activities, involve frequent and more or less regular readjustment in direction. The results here reported would, therefore, apply to these movements.

A movement of the kind under consideration, then, involves not only a continual change in position but also a frequent, and in the case of the circular movement, a continual readjustment of direction. The character of the innervation, the muscular coordination and the direction of application of energy is ever changing. The inhibitory impulse and the type of muscular activity necessary to stop the movement must be different for every change in the movement. Stopping a movement is not a passive affair nor is it a uniform inhibitory process which may be imposed alike on the movement at any point. It is rather an active process, the precise character of which is determined by the particular form of movement to be stopped. It involves a withdrawal of innervation from the muscles which produce the movement and an innervation in turn of the antagonistic muscles. From the point of view either of consciousness or of the physiological mechanism, then, no preparation of a general sort can be made for stopping a movement, such as that involved in the experiment. The attention can not be directed to any point in the movement nor to any specific motor adjustment. No muscles can be innervated nor any nervous path ‘ set ’ in preparation for the stopping.

With the starting reaction, however, the case is different. The process of preparation for a reaction by a partial innervation of the muscles which produce the movement and a ‘ set ’

of the nervous path is too familiar to need more than mention. It is easy to understand from the greater complexity of the stopping reactions and the fact that they permit of no preparation why they require more time than the starting reactions.

The qualitative records of these reactions bear out this statement of the difference between the starting and stopping reactions. The first response to the signal in stopping a movement is a diffuse spreading of the innervation, rather than a precise reaction along well-defined paths. This causes in the majority of cases at the same time a decrease in the speed and amplitude of the movement which is being made and an increase in the pressure exerted. In contrast to this, in the starting reactions a decrease in pressure often came at first, showing that the innervation is applied directly to the muscles involved in the precise reactions. When the movement was stopped the energy was not withdrawn but was transferred by an elaborate readjustment to another set of muscles.

If the explanation offered of the difference in the time of stopping and starting a movement is correct, we should expect that changing the direction of a movement would also be slow, for changing the direction of a movement consists in some cases, as was pointed out in the description of the records, in first stopping the movement and then beginning a new one in a different direction. The complete process already described as characteristic of stopping a movement had to take place and an additional activity had to be inaugurated to produce the new line. The case is further complicated by the fact that the particular activity which is required to produce this new movement is dependent upon the point in the earlier movement at which reaction happens to take place. Sometimes, it is true, the reactor seems to be able to eliminate the stopping process to a certain extent, and make the horizontal movement continuous with the vertical, without a pause; but the effort is usually only partly successful, as shown by the fact that the new movement starts not horizontally, but at an acute angle from the vertical line, indicating that the innervation into the old channels was not entirely inhibited before the new impulse began to act. In any case this process would be as complex as that of stopping

a movement, the coordination producing the new movement being substituted for the coordination which produced the inhibition of the movement.

The results in all these cases of movements turned into a new direction should be recognized as results with reactors who were not especially expert in the particular kind of movement here undertaken. If the explanations are to be applied to long-practiced activities, such as those of writing, it should be recognized that the succession of innervations is much less delayed after practice. There is even then a necessity of stopping the old reaction and beginning a new reaction, but the well-trained coordinations show little of the diffuse period. Indeed there were some cases of reaction by changing the direction of a movement in which the diffuse period was much shorter than usual, but it was nearly always long enough to indicate that the reaction was more complex than a simple stopping reaction. Among the results reported in this paper the descriptions given of the transitions from one part of a circle to another, or from an upward line to a downward line are better examples of well-trained and fully prepared transitions in movement.

The greater length of the reactions in drawing a line to a point or in making a geometrical figure would seem to be susceptible of a similar explanation. In these cases the initial movement was definite enough, but the attention was divided between the initial movement and the later stopping of the movement. The movement was not only to be made, it was also to be stopped or its direction changed at a certain place. This fact must have resulted not only in the innervation of the muscles employed in the initial movement, but also in the partial innervation of the muscles used in the subsequent stopping or change in direction, and this must have interfered with the rapidity of the reaction.

The analysis of movements undertaken in this investigation shows that there are a great variety of readjustments which are not fully and explicitly recognized by the reactor. This can be brought out by reporting certain introspections which the reactors gave during the series. For example, one reactor introspected the vertical starting movements,

especially the upward movements, as slower than the stopping movements. On the contrary, the starting movements were the faster and of these the upward movements were the faster. Again a reaction was sometimes introspected as slow when the reaction itself was fast, but the movement following the reaction was slow. One reactor, for example, said that, in Series III., stopping a vertical movement, he always stopped before the stroke was finished which he was making when the signal came, provided the signal came near the beginning of the stroke. If the signal came past the first part of the stroke he said he stopped within the next stroke. As a matter of fact he in no case stopped before he had made two strokes after the signal.

Again, the pressure changes were not recognized as pressure changes. Though they were constantly present the reactors were for the most part wholly unaware of them; they existed for the reactor's consciousness not as pressure changes but as changes in the position and character of the movement.

This confusion of the factors involved in movement is explained if we regard the function of consciousness as not primarily to mirror the separate elements of movement, but to grasp it as a unit, though a progressively changing one.

The reactor was not conscious of changes in the speed and amplitude of movement, or of the changes in intensity of pressure which have been described as occurring between the signal and the reaction. These were merged for consciousness into the perception of the reaction. The changes in speed and amplitude and the final stopping made up the change of balance between rest and motion which constituted the reaction, but only the total change and not the gradations from motion to rest were apprehended.

The results of this investigation emphasize the unity of a reaction process and emphasize further the complete parallelism between the conscious attitude as a whole to the reaction complex as a whole, while showing clearly that consciousness does not reflect in detail the factors of the reaction.

REACTIONS TO EQUAL WEIGHTS OF UNEQUAL SIZE.

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This paper describes a method of recording the movements which are executed in lifting two boxes of equal weight but unequal size. It was found from records thus taken that the small box is usually raised later than the large box and that less energy is expended in lifting the small box than in lifting the large one. Examples of the records are shown and the results are discussed in detail.

The illusion which results when an observer lifts two blocks of unequal size, but equal objective weight, has been the subject of much investigation. So far as the writer is able to discover, however, no attempt has been made to secure direct records of the muscular reactions accompanying the illusion. With this end in view, a large number of records were taken with thirty-six reactors, most of whom were not familiar with the illusion or its explanation. The total number of records referred to in the present paper is about 400.

The apparatus employed in securing records is represented in Fig. 89, a large box *A,* 22.7 x 22.7 x 22.4 cm. outside measurement, weighing 352 grams rested on the platform *P.* Beside box *A* was a shelf *S* on which rested a small box *B.* The shelf was just high enough to bring the top of the small box *B* to the same level as the top of the large box *A.* Box *B* was decidedly smaller than box *A,* being only 3.7 x 3.8 x 4 cm., yet was weighted so as to equal in weight the large box *A;* it weighed therefore 352 grams. From the center of crosspieces in the bottoms of the boxes, linen threads *CC* passed down through holes in the platform, over pulleys *YY,* to small weights not shown in the figure, but attached to the threads at *W* and *W.* The weights were each 57 grams and served to keep the thread under tension when the boxes were placed on the platform. Between the pulleys *YY,* the threads *CC* were attached to the arms of levers *LL.* Boxes *A* and *B* could

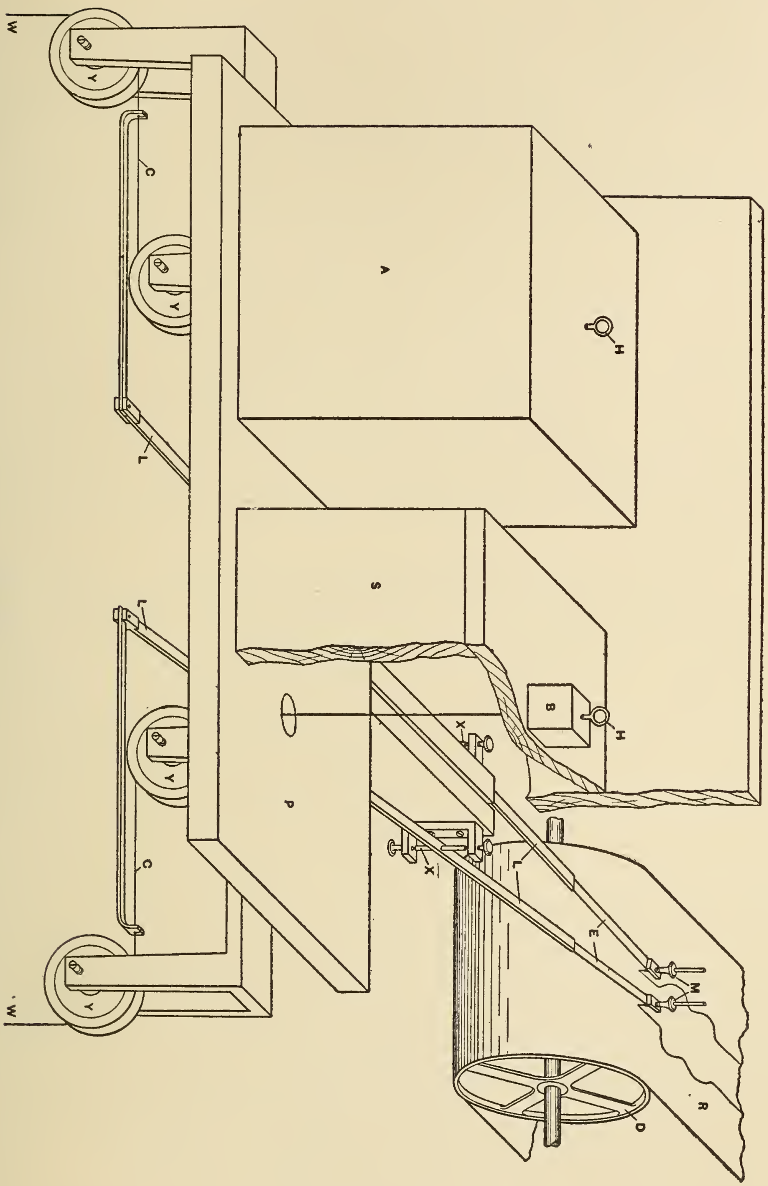

FIG. 89.

be interchanged quickly because of the simple fastening device in each of the boxes, and the fact that the shelf *S* was made so as to be set up on either end of the platform. This interchangeableness of the boxes made it possible to secure right and left-hand trials with the boxes. The levers *LL* passed under the platform *P* and appeared beyond, as shown in the figure. At *XX* the levers were pivoted so as to move in the same horizontal plane. At the extremities of the levers elastic strips of metal *E* continued the length of the levers to *M*. At *M* pencils passed through adjustable clutches. The pencil tips bore on a paper belt *R*, 15 cm. wide, which traveled over the top of a drum *D*. The elastic metal strips *E* were inserted to secure constant contact of the pencils' points with the moving paper and at the same time to prevent excessive friction between the pencils and the paper. Each box was provided with a screw eye *HH*, so that the subject could easily grasp it when he lifted the box and would have the same kind of contact with both boxes.

When the boxes were at rest on the platform and shelf the two pencil points traced parallel lines on the paper *R*, as indicated at the beginning and end of the record shown in the drawing. When the boxes were lifted the pencil points moved away from each other and traced curves which showed the distance to which the boxes were lifted, and the exact moment at which they began to rise and at which they were replaced. The curves also show all vertical movements of the boxes while they are held in the hand. The compound levers prevented any binding of the string during the movement of the levers.

The subjects stood facing the boxes *A* and *B*. By means of a pasteboard screen not shown in the figure the levers and moving paper ribbon were hidden from view. The subject was brought into such a position with reference to the boxes that his arms were extended during the lifting in an easy horizontal position. The subject was instructed to grasp the boxes *A* and *B* by the screw eyes *HH* between forefinger and thumb, in as nearly the same manner as possible; to raise them in any way preferred; to estimate their relative weight and then to replace them. In the majority of cases, the subject reported

after the trial his experience with the boxes, in such terms as the following: "The smaller is twice as heavy as the larger one." "They seem this time more nearly alike." "They are coming nearer together." "The difference between them is increasing; the smaller is about six times as heavy as the larger one now." "They are nothing alike, but I can't tell how much heavier one is than the other."

In Fig. 90 is shown a record taken from the series of subjects No. 21. The arrow points in the direction in which the

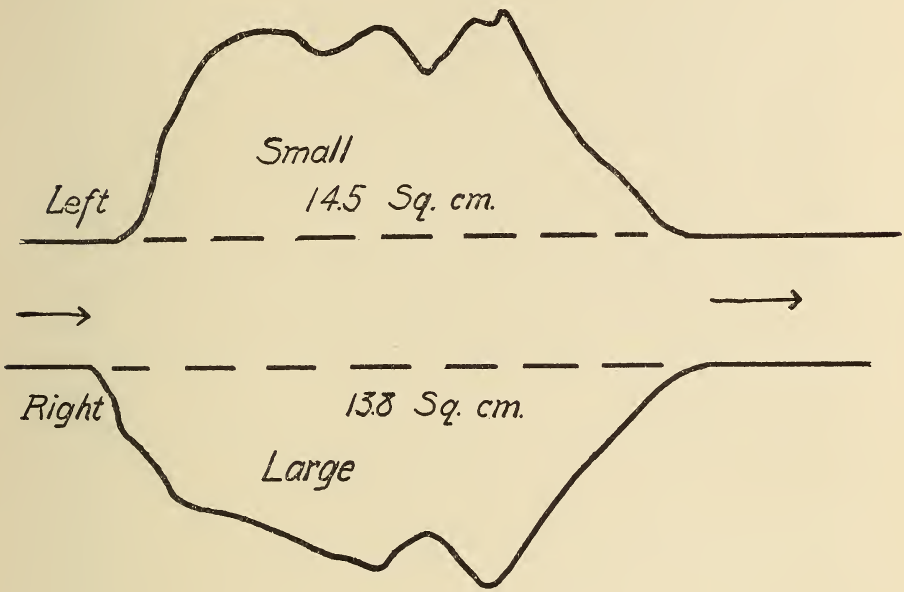

Fig. 90.

record was made. The words 'right' and 'left' indicate right and left-hand records respectively. 'Small' and 'large' refer to boxes *B* and *A*. In the record it is clearly evident that the large box was first raised because the record for this box leaves the horizontal before the record for the small box. Furthermore, it will be seen that there is some difference between the amount of lifting in the two cases. In order to secure a quantitative basis for comparison in this matter, the following method has been used. Base lines, shown by the dotted lines in the figure, were drawn from the point where the

box was first disturbed, as shown by the lines losing their evenness and horizontal direction, to the point where the boxes were replaced, as shown by the fact that the lines again assumed an even and horizontal character. By means of a planimeter the areas enclosed by the base lines and the curves of the records were measured. The enclosed areas are in direct proportion to the amount of energy expended by the subject in lifting the boxes while judging their weight. In the case shown in Fig. 90, the amount of muscular energy expended while judging the small box is proportional to the area of the upper curve which is 14.5 square centimeters. In like manner 13.8 sq. cm., the area of the lower curve, represents the expenditure of energy in estimating the large box. Allowing 100% to stand for the total expenditure while judging both boxes, 51% was expended in estimating the small box and 49% in estimating the large box. Many of the curves show a much greater difference and most show a greater area for the large box, as will be made clear in the tables to be given later. This curve is the second in the series and the subject was influenced by the observation made in the first test in which the small box seemed heavier than the large box.

The number of trials obtained from each subject varied greatly; in the case of two subjects going as high as thirty and in no case falling below three. The great majority of the subjects made from five to seven trials in each series. On approaching the apparatus for the first series of trials the subject was in nearly every instance unacquainted with the illusion. He was given no information about the experiment, but was merely told to lift the boxes and determine, if possible, their relative weights. Furthermore, he was asked to give his impressions while making the trial. In not more than two cases was the illusion known to the subject, or explained to him before he began. The results of these subjects were not essentially different from those of the subjects unacquainted with the illusion and consequently they are not separately treated. Sixteen of the thirty-six subjects gave on later dates a second series of trials under conditions similar to those of the first series except that they had in the second series the benefit of such experience as they gained in the first series.

In both the first and second series a number of cases were introduced in which the subject after lifting the boxes in the natural way with open eyes, lifted the boxes from three to sixteen times in succession with eyes closed. During these series, which will be designated as closed-eye series, the subjects did not look at the boxes at all. Whatever illusion they experienced depended upon the memory which they had of the earlier visual perception of the boxes. Twenty-one such closed-eye series were obtained.

In certain cases the apparatus was set up in such a way as to present the large box to the right hand, in others so as to present the small box to the right hand. A general survey of the records does not show any characteristic differences for these cases.

Thirty-one of the thirty-six subjects, at their first trial, pronounced the small box the heavier; three said the boxes were equal; one regarded the large box as heavier; one was suspicious that by some magnetic or electrical contrivance the weights of the boxes were made to vary while in his grasp.

Turning now to the way in which the boxes were raised, or as we shall hereafter call it, the mode of attack, it is perfectly clear that in the great majority of cases the large box was

TABLE I.

A.

Trials.	1	2	3	4	5	6	7
Large.	33	28	24	20	17	10	4
Small.	1	5	7	4	4	4	2
Equal.	2	3	5	5	7	5	3

B.

Trials.	1	2	3	4	5	6
Large.	10	10	11	7	7	4
Small.	2	0	0	2	2	1
Equal.	1	3	2	4	3	3

C.

Trials.	1	2	3	4	5
Large.	9	11	11	8	8
Small.	4	4	2	2	0
Equal.	3	1	3	3	4

raised before the small. This is shown in the curve reproduced in Fig. 90 and in the first curve in Fig. 92. Table I. shows the total number of cases for all of the subjects, in which each of the boxes was first attacked, and the number of cases in which no measurable difference in the time of attacks appeared. The quantities in the horizontal columns marked 'Large' show the number of cases in which the large box was first attacked. 'Small' indicates in like manner the number of first attacks of the small box, and 'equal' indicates simultaneous attack. The table shows the distribution of 'first attacks' in successive trials. Thus, under 1 the results are shown for the first time each of the subjects took up the boxes. The quantities under 2 show the modes of attack in the second trial, and so on. The total number in each vertical column shows how many subjects made that trial. Thus, in Table I., *A*, thirty-six subjects made three trials, twenty-nine made four trials, twenty-eight made five trials, and so on. The three parts of Table I. show the results for the first series of trials (*A*), for the second series (*B*), and for the tests with closed eyes whether taken with the first series or second (*C*).

The same results are presented in Table II. in percentages. Thus thirty-three cases of first attacks of the large box in the

TABLE II.

A.

Trials.	1	2	3	4	5	6	7
Large.	92	78	67	69	61	53	45
Small.	3	14	19	14	14	21	22
Equal.	5	8	14	17	25	26	33

B.

Trials.	1	2	3	4	5	6
Large.	77	77	85	54	58	50
Small.	15	0	0	15	17	13
Equal.	8	23	15	31	25	37

C.

Trials.	1	2	3	4	5
Large.	56	69	69	62	67
Small.	25	25	12	15	0
Equal.	19	6	19	23	33

first series are 92% of the total number of such cases obtained from all subjects. The percentage tables are used for the curves in Fig. 91.

Before turning to a discussion of these tables and curves,

FIG. 91.

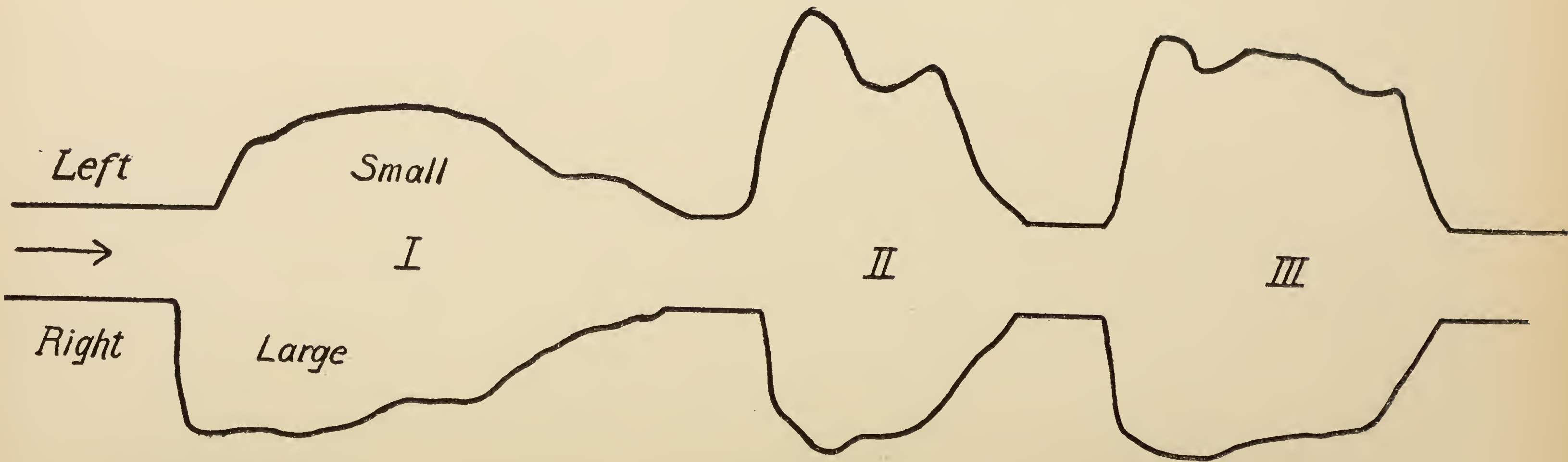

FIG. 92.

it may be well to present a series of records which show very strikingly the three modes of attack. Such a series of records is reproduced in Fig. 92. These are records of the first, second and third trials in the first series of subject 29. It is to be noted that after a first trial which gives very clearly the typical mode of attack of the large box first, the second and third trials show a readjustment in the mode of the attack. This readjustment results in the second trial in a vigorous attack upon the small box which was much behind in the first trial. There can be little doubt that the motive of this readjustment is to be sought in the experience gained in the first test. The subject is no longer dependent upon his visual experience alone. He has, in the second trial, the results of the first test in which he found the small box heavier than the large box. He consequently attacks the box which he believes to be heavier with greater vigor. The third trial shows a tendency, as contrasted with the first and second, to make the attack less a matter of previous adjustment. The boxes are now picked up together and judged afterwards. Every record after the first is a complex. In spite of this, the table shows that the large box continued throughout to be in general the box first attacked.

Returning to the discussion of Tables I. and II. and the curves in Fig. 91, it will be seen that the percentage of attacks on the large box is throughout the highest. It falls off steadily, however, in the successive trials under the influences discussed in the last paragraph. The number of first attacks on the small box changes more irregularly in the successive trials. There is a general tendency towards more frequent first attacks of this box, but the irregular curve shows clearly that this tendency is complicated by other factors. Especially notable is the fact that in the second column of Table *A* there is an enormous increase in the percentage of small box attacks. The influence of the experience obtained in the first test is thus clearly evidenced. The simultaneous attacking of both boxes increases steadily throughout the trials. This tendency to equalize is evidence of a more deliberate and well balanced preparation for the judgment. In the second series of trials reported in *B* and secured from certain of the subjects reported in the first

series on days subsequent to that of the first series, the same general tendency is displayed in the curves for the large box, and in the curve of equal attack. The curve for the small box displays even greater irregularity than in the first series. This last-mentioned curve begins at 15% and instead of rising to a higher point as in the first series, it drops immediately to zero and remains at zero until the fourth trial when it rises to 15%. In the trials in which the eyes were closed (Closed-eye Series), the striking fact is that the directions of the large and small box curves are reversed in direction from the similar curves in the first and second series, yet the curve of equal attacks has the same general upward movement, though less regular.

Turning from the question of the time of attack to the areas of the curves, it was found that on the average the areas of the curves for the small box were less than the areas of the

TABLE III.

Trials.		1	2	3	4	5	6
A	Small.	10.9	9.1	8.6	7.0	8.6	7.6
	Large.	10.4	10.4	11.3	8.0	8.4	8.4
B	Small.	13.0	24.9	78.4	27.5	28.2	27.0
	Large.	16.5	27.3	85.4	25.9	37.6	36.9
C	Small.	7.3	25.1	12.2	13.0	26.3	9.9
	Large.	6.4	24.0	12.6	13.1	17.5	11.9
D	Small.	8.7	9.5	11.3	6.1	6.6	9.8
	Large.	5.4	11.8	17.9	8.5	16.8	10.0
E	Small.	25.9	3.2	7.1	4.6	14.0	10.4
	Large.	33.9	13.6	11.1	9.1	25.0	14.9

curves for the large box. Table III. shows the record in detail for five subjects, each of whom made six trials. The quantities are square centimeters. It will be seen from this table that there are many exceptions to the general rule, frequent cases appearing in which the area is greatest for the small box. Attention should not be distracted by these exceptions from the general result which is unequivocal. It should be remembered that the individual cases are complicated series of adjustments rather than single acts. The curves are often spread out over distances of ten centimeters or more and are very irregular in form. Indeed, the curves vary so much in form and area that it was found to be advantageous to reduce all areas to a percentage basis, as indicated in connection with

Fig. 90. The areas of the small box and large box curves for a given subject in a given trial were added together and treated as 100%. The area of each curve was then expressed as a part of the total 100%. The percentages thus obtained were averaged for the thirty-six subjects with the results reported in the horizontal columns marked 'Per cent. large' and 'Per cent. small' in Table IV. Subtracting the percentages for the

TABLE IV.

FIRST SERIES.

A.

Trials.	1	2	3	4	5	6
Per cent. small.	44	44	43	46	45	47
Per cent. large.	56	56	57	54	55	53
Difference.	12	12	14	8	10	6

SECOND SERIES.

B.

Per cent. small.	46	47	48	47	47	47
Per cent. large.	54	53	52	53	53	53
Difference.	8	6	4	6	6	6

THIRD SERIES (EYES CLOSED).

C.

Per cent. small.	45	42	45	43	47	43
Per cent. large.	55	58	55	57	53	57
Difference.	10	16	10	14	6	14

small box from those of the large box, we obtain results which are reported in the column marked 'Difference.' From these differences is plotted the curve shown in Fig. 93. It will be noted that in every case the average area for the large box is greater than that for the small box. It will be noted further that this greater area for the large box steadily decreases throughout the first series and that it is in general lower in the second series than in the first. As in the matter of 'attack,' the closed-eye series is irregular as might be expected from the complex conditions under which these tests were taken and their irregular distribution through the first and second day's trials.

In addition to the order of attack and area, the curves show many characteristics which invite further attention. The number of curves from each subject is relatively small, however,

and it seems desirable to obtain longer series with each subject before attempting to formulate results regarding the character of the curves. In spite of the small number of records from each of the subjects, it was evident in a number of cases that the subject showed an individual mode of reaction to the boxes. This individual mode of reaction showed variations in successive trials, but tended to recur even in the short series taken.

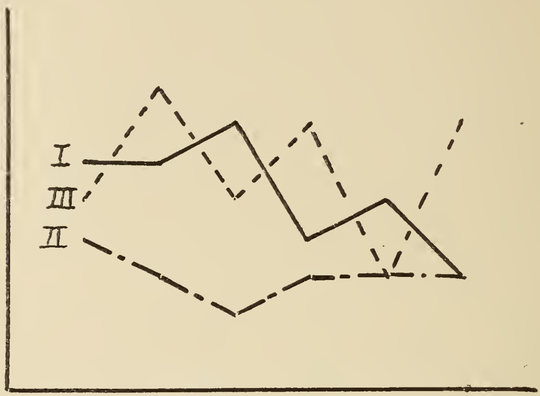

FIG. 93.

There were also differences between the reactions to the large and small boxes; the two boxes being in many cases lifted in typically different ways. There was, however, such a relationship between the two curves in a given trial that when one curve varied from that of the preceding trial, an accompanying change of some kind appeared in the parallel record for the other box. More records are being collected and a subsequent report will deal with the form of the curves.

All of the results obtained go to show that a subject has much greater muscular tension in the hand which lifts the large box than in the hand which lifts the small box. This greater muscular tension is the direct expression of an organized habit of response to familiar visual objects. So far as any subject has had experience with boxes made of like material, he has found in general, that the larger the box the more the energy required to lift it. This organized tendency of expression is an essential part of the perceptual process. The muscular

tension is not due to any voluntary effort, but is the motor phase of a total sensory-motor adjustment which is the perceptual process. The naive sensory-motor adjustment is disturbed, as shown by the tables even in the second test. After the first attack in the first series, when the great muscular effort expended on the large box resulted in the striking experience that the large box seemed too light, there began a shifting of attention and at the same time a modification of the muscular adjustment. That is, the subject failing to realize the expected resistance from the small and large boxes, began to modify at once his perceptual experience and his muscular tension. This tendency to readjust shows itself, as pointed out above, in a marked increase in the number of cases in which the small box is lifted first. Fig. 92 shows the character of such a readjustment very clearly. The most striking evidence of the tendency to readjust the whole muscular reaction appears in the facts shown in all the tables and curves, that the cases of simultaneous attack increase steadily from trial to trial and the difference in the areas of the curves for the large and small boxes gradually tends to disappear. In individual cases the corrective tendencies are so strong that the small box is frequently attacked for one or more trials more vigorously than the large box.

The apparent disappearance of distinctive motor adjustments to the two boxes in the course of the experiments is not to be misunderstood as showing an independence of the motor adjustment from the perceptual experience. For, in the first place, many of the subjects gave the judgment equality or even less weight for the small box in the course of the series. Above all, every subject had after the first trial, as has been repeatedly pointed out, the direct testimony of his own experience that the small was not lighter than the large box. When any subject began in the second, and succeeding trials, to pick up the boxes, his perceptual process was influenced to some extent by his experience in the first trial. The fundamental habit of reaction to the two boxes was not entirely overcome, however, as is shown by the general fact that on the average greater energy was expended on the large box throughout the whole series.

The irregularities in form of movement which appear in the second and in subsequent trials are consequently to be related directly to the conflict in perceptual experience between the natural tendency to react differently to the two boxes and the experience acquired in previous trials. The intimate relation between perception and motor adjustment is consequently clearly shown by all the results.

STUDIES IN PERCEPTUAL DEVELOPMENT.

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This paper reports the development shown by a number of subjects in the course of a series of tests in which they attempted to reproduce a simple figure which had been shown to them for ten seconds. The reproductions were made under varying conditions; sometimes with the eyes closed, sometimes with the result covered so that only the hand movement used in drawing could be seen, sometimes with both the movement and results of the drawing clearly visible. It is shown by a comparison of these results that the recognition of the figure is gradual, some subjects beginning at the beginning of the figure and working it out in detail, others beginning at other points in the figure. It is shown that the greatest amount of error is in the middle of the figure. It is shown that there is a difference in the rate of mastery of the size and relative position of the lines. It is shown that in certain cases parts of the figure may improve for a time, and the same parts may later be partially or completely overlooked. The series of tests is available for a demonstration of the improvement which arises from practice in a very short interval.

The purpose and character of the tests to be reported in this paper can best be made clear by a brief consideration of certain of the different types of mental development included under the general term, learning. It is evident that the process of learning the meaning of a word is very different in character from the process of learning to use a tool. Learning to repeat a list of names is essentially different in its elements and development from learning to draw. These illustrations show that the student of psychology must devote himself to a great variety of different investigations if he would make an exhaustive experimental study of learning processes. Thus, it must be recognized that the results gained from an investigation of some habit of manual dexterity are not forthwith applicable to other spheres of learning. Without attempting to formulate any complete classification of learning processes it may be said that the present tests deal with the progressive mastery of certain simple percepts. The motor processes which were of

necessity involved in securing the results did not change appreciably in the course of the tests. The ideas which the subjects had were, indeed, considered and recorded, but they were not the matters of chief interest to either the subjects or the experimenters and they did not undergo any significant modification in the course of the tests. The memory which was involved in carrying out the experiment was not variable, except as it varies in successive stages of any perceptual process.

The outcome of the investigation must serve as the justification for the selection of such a problem, but it will not be out of place to state at the outset some of the considerations which prompted the writers to take up the study of this special phase of learning. In the first place, little or no experimental work has been done in this special field. Many forms of motor practice have been examined and many complex processes such as those involved in remembering ideas and mastering more or less complex systems of ideas have also been investigated. Percepts have, indeed, been thoroughly examined by purely analytical methods, but the application of genetic methods to this type of experience has been relatively uncommon. In the second place, the writers were convinced from a review of the few results which have been secured through a genetic examination of percepts, that the theory of perception itself, as well as the theory of the learning process, would be advanced by an examination of the changes due to increased familiarity with simple percepts. The genetic method is advantageous as a means of analysis, for whenever a change appears in conscious experience as a result of practice, the elements of the experience are sure to undergo a rearrangement of such a character that they will be more easily discovered than a relatively static experience which is undergoing no marked development. In the third place, there was a practical motive in the present investigation. It is very desirable that a number of simple tests be devised which shall furnish the student in a reasonably short period with definite concrete material on which he may base some preliminary study of mental development. A single laboratory period is not adequate time in which to secure a significant series of improvements in

most habits. Something can be done in the line of motor development if very crude forms of movement are taken and the results are reduced to a type of record which makes possible exact measurements. But even under the most favorable conditions changes in motor processes are not very marked in a single laboratory exercise. Nor are the experiments with memory or association especially favorable single exercises. The tests with percepts, on the other hand, offer a very favorable opportunity for the study of some of the most typical forms of mental development within the compass of a single laboratory period.

The method of these tests was as follows. A simple figure made up of straight and curved lines was exposed to the subject's view for a period of ten seconds and then covered up. The subject was required immediately to draw the figure as he had recognized it. The figure was then exposed for another ten seconds and the subject made another drawing. This continued for ten or more times with the same figure. Three different types of conditions were imposed upon the subject during the drawing. First, he was required immediately after the pattern was covered to close his eyes and draw the reproduction without seeing his movements or the drawing which he produced. Second, he was allowed to see his movements, but not the drawing which he produced. This was accomplished by requiring him to trace with a dull metallic point on carbon paper. The carbon paper was, before the beginning of the experiment, defaced by a great number of lines, so that the special marks made in the tracing of the figure were not distinguishable. The figure traced on the blank paper under the carbon paper served as a perfectly clear record of the movement, but this figure was not seen by the subject himself. Third, the subject was allowed to see both movement and resulting drawing. The three cases will be designated in all tables and references as follows: *S* (shut), *C* (carbon paper) and *O* (open). Various other modifications were introduced in a few of the tests, such for example as giving a subject who was drawing with his eyes closed some verbal criticisms of his results, allowing the subject to examine the resulting drawing

after it was completed, but not while it was in process of making. Other modifications may be suggested, such, for example, as allowing the subject to draw with closed eyes on soft paper and to trace with a finger of the left hand the line which has been produced in the drawing.

The subjects whose results will be utilized for this paper were members of a class in educational psychology and advanced students in the regular laboratory course. They will be designated by single letters, a given letter referring in each case to the same subject.

The writers have encountered a great deal of difficulty in preparing the results of these experiments for concise presentation. When a series of ten drawings is laid before the experimenter and the successive efforts of the subject to reproduce the pattern are compared, there is often very striking evidence of the development which has taken place during the ten drawings, but it is extremely difficult to prepare a table or curve which will adequately demonstrate these facts of development. In order to make clear the character of the results and at the same time illustrate some of the most significant characteristics of the drawing, a number of series of drawings are reproduced in Figs 94 and 95. These reproductions are one sixteenth the size of the actual drawings. Fig. 94 shows two full series with subjects *F* and *W*, both drawing in this case with eyes closed. Above the two series of drawings from these subjects, is shown the pattern which they were trying to reproduce.

Considering the series *A*, which is from subject *F*, certain facts regarding the subject's perceptual development are very evident. The first drawing is correct in general outline, but very vague in its details. The subject has here a percept comparable to that which most persons have of an object which has been observed superficially but not examined in detail. The face of a comparative stranger, for example, or the form of a plant or wall paper pattern are first recognized in gross general outline. The second drawing shows progress in that the details now begin to be correctly reproduced. The first part of the figure has evidently received not merely a vague general inspection, but has been examined in detail. The slow

rate at which the details of a percept are recognized is here strikingly illustrated in the fact that an adult who is perfectly familiar with lines of this character does not succeed in ten seconds in clearing up more than five lines. Furthermore, the fourth and fifth lines are sufficiently different from the pattern to be recognized as rough approximations rather than fully recognized details. The general form of the figure is main-

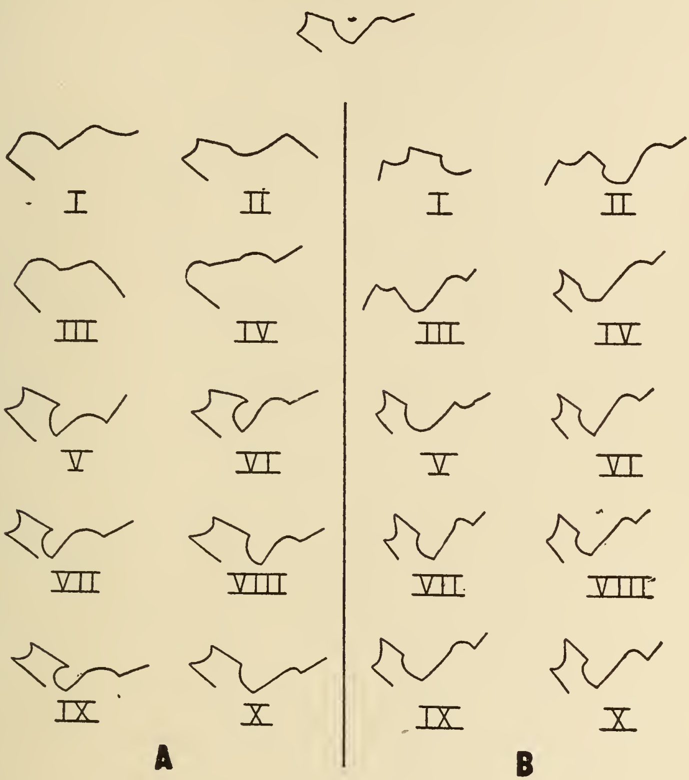


Fig. 94.

tained while the details of the first part are being worked out. In drawing 3 is illustrated a fact which comes out time and time again with almost every subject. There appears in the course of perceptual development a certain point where readjustment of the recognition of the parts is so actively under way that if the subject is interrupted before the readjustment

is complete, the reproduction shows the greatest confusion. Thus, in drawing 3 what had been gained in drawing 2 seems to be wholly lost. Moreover, the general form of the figure which was approximated in the first drawing is here much less correctly reproduced than in either of the preceding figures. Such a poor reproduction as that in drawing 3 must be recognized as very striking evidence of the complexity of the perceptual process. The explanation of the period of confusion can be made out very clearly in this case by an examination of drawing 4 and reference to the subject's introspections. The introspective record is as follows: "There is a succession of straight and curved lines, but their order is very confused in my mind. I think there should be more curves especially at the end." The essential point is in this reference to the end. From drawing 4 it is evident that the subject is trying to straighten out the confusion at the right end. The right end of the figure was vague in drawings 1 and 2. In 2 the first part of the figure was mastered. In turning to the right end the confusion arises, as shown in drawing 3. The subject has not had time in drawing 3 to master the right end. This was finally accomplished in drawing 4, but the attention is withdrawn from the general form of the figure and from the first part of the drawing in the effort to work out the last part. Drawing 3 is a very striking example of the difficulty of any single test of mental ability. Without drawings 1, 2, and 4, a very false notion would be gained of the subject's mental condition from drawing 3.

Drawing 4 shows, as has been pointed out, the mastery of the end of the drawing. It also shows that the subject has mastered a general characteristic of the figure which consists in the alternation of curved and straight lines. The introspective records show that this principle has been explicitly recognized. That the perceptual record of the first part of the figure is wholly incorrect shows what is certainly a general tendency in adult mental processes, namely, the tendency to generalize perceptual experiences under some abstract statement and neglect the perceptual details out of which the abstract statement grew. The subject of these tests must have

seen the succession of straight lines and curves, but was evidently more attracted by the abstract relational fact of succession than by the concrete forms of the parts of the figure. The concrete relations were recognized only at the end of the figure towards which perceptual attention had been definitely attracted.

Drawing 5 shows the mastery of the figure. Its relation to the earlier processes of distribution of attention, of mastery of parts, of confusion and recognition of the general principle of alternation is sufficiently obvious from what has been said. This drawing could not be understood at all if it had been preceded merely by drawing 4 or by 3 and 4. The mastery of the first part of the drawing, as shown in drawing 2, is an essential part of the preparation for drawing 5. Though the elements mastered in drawing 2 have been for a time neglected in drawing 3 and 4, they can be more easily recovered than at first. In drawing 2 there is evidence that if the subject attended to the first part of the figure, he could not at the same time include in a single process of recognition the last part of the figure. Putting the matter in quantitative terms, we may say that the scope of perceptual consciousness, as evidenced in drawing 2, is three clear lines and two vague lines. In drawing 5 the first five lines are recovered with sufficient ease so that the scope of perceptual consciousness extends over all seven lines. The greater inclusiveness of perceptual consciousness in drawing 5 gives evidence of a facilitation in some degree of the perception of the first five lines, and since this facilitation did not occur during the period occupied in drawings 3 and 4, it must have been carried over from the period of drawing 2.

The remaining drawings of this series show certain details which are typical. Thus the angle of line three deteriorates instead of improves through the later drawings of the series. This may be connected with the fact that the greatest error in all the later drawings is to be found in the angle of line four. There is very noticeable variation in the position of line four. In drawing 8 it is better than in drawing 7, but in 9 it is again worse than in drawing 8. In drawing 10 there is a very decided improvement in the position of line four. There is, on

the other hand, in drawing 10, not merely the deterioration of line three noted above, but also a very marked lapse in the length of line six. Other similar facts will be obvious from the figures.

The various lapses and slight improvements in the last five drawings show very clearly why there is so little improvement in our ordinary perception of complex figures. Attention is from moment to moment fastened upon this or that detail of the figure and there is a corresponding withdrawal of attention from some other part. The complete mastery of all the details is therefore a long process. In most ordinary experiences the interval between observations is so long that the lapses more than make up for the periods of improvement and so we have merely crude approximations to complete and correct percepts.

Another general fact shown by the series as a whole is that the size of the figures is throughout too large. Indeed, there was a very general tendency, as will be shown later, on the part of all the subjects to make mistakes in the size of the drawings. The significant fact in this immediate connection is that the subject was in no case conscious of the error in size. That is evidently a matter upon which attention must be especially directed. While attention is on the various lines and their positions, there is no attention for the characteristic of size.

Some use has been made of the introspective record. In all of the experiments the subjects were allowed to make whatever comments they would make voluntarily. They were not questioned because that would have served to concentrate the attention of the drawer upon matters in which the experimenter might be interested. The voluntary observations were very meager on the whole. The process of perceptual recognition was not to any great extent clear to the subjects. This would seem to indicate that ideas about one's own perceptual processes constitute, as does size, yet another separate item of possible attention. While attention is concentrated on the lines of the figure, one is not observing the direction of attention itself. This is merely a concrete example of the criticism often made of the purely introspective method. Reference to the criticism here is justified both by the concrete example furnished

by these experiments and by the fact that an emphatic assertion of the distinction between perception and ideation is required for an intelligent understanding of the results themselves, as was made clear in the discussion of drawing 3.

It may be well to pause in the description of these results and meet a possible objection which may be raised to the explanations. It may be said by some that the variations in the figures are due to memory conditions rather than to purely perceptual processes. The experiments, it will then be said, are tests of memory rather than of perception. In a certain measure it must be recognized that memory is involved in all these tests. But there is no complex percept which can be built up without memory. It is clear from our examination of drawing 5 in Fig. 94, *A,* that the effects of earlier stages of the perceptual process are clearly present in the fifth stage. The complete recognition of figures is thus a gradual development which involves a certain amount of retention. The retention will, however, indicate by its content and fullness the character of the perceptual process. Thus it is obvious in drawing 2 that the last part of the figure was not as clearly apprehended as the first part. It makes no difference whether the statement is made with reference to the retention or the original recognition. This is perfectly clear when it is noted that in drawing 4 the emphasis is obviously on the last part of the figure rather than the first. Drawing 5 shows the necessity of considering in any discussion of perception the retention of earlier influences and at the same time shows the purely perceptual problem of the tests, for while the interval between the inspection of the pattern and the drawing is the same as in earlier cases, the product is totally different in character and shows clearly the dependence of the product upon the clearness and fullness of mastery of the figure. The tests should not be criticized because they involve memory, it should rather be recognized that all perception involves memory, the memory phase being in general overlooked by any purely analytical method of examining experience.

Turning to series *B* in Fig. 94, we find an entirely different type of development from that shown in series *A*. Instead

of beginning with a recognition of the general form of the figure and neglecting details as did the subject in series *A*, this subject began by making an effort to master the details as they appear at the beginning of the figure. The subject's introspection is: "Too many lines. Couldn't get the direction of the lines after the first three." Drawing 2 shows that the attention was devoted to filling out the figure, even though the image of the first part was left with all of its original defects. The introspection is as follows: "Saw distinctly a straight line, then a curve, alternating four of each." This is a vague ideational formulation of the principle of the figure. As a matter of perception the number of straight lines is correctly given, though the curves are not correctly reported.

The further development of this series need not be discussed in detail except to call attention to the lapse in line six of drawing 5. In drawing 4 there is clear evidence that the subject was interested chiefly in the early part of the figure. The introspections for drawings 4 and 5 show this concentration of attention on the first part of the figure. After drawing 4 the subject records: "Positive about number of curved and straight lines, but direction of second curve vague. After a little effort more distinct." After drawing 5 the subject records: "Direction of curved line after second straight line vague." Since the attention was on the first part of the figure, the curve toward the end was neglected with the resulting error shown in drawing 5.

It will be noticed that in point of size figures 9 and 10 are among the worst in the series. This shows again that while one phase of experience is improving other phases may deteriorate.

The records reproduced in Fig. 94 show that the most radical changes in the records occur in the first part of each series. This is true in general of all series for all subjects. The number of lines in the pattern might have been increased and then the serious errors would have appeared through a larger part of the series. The conditions being as they are, we can present most of the striking material in a number of series in a single figure, by reproducing only the first five drawings in each of

four series from different subjects. The series *A* from subject *O* was made with carbon paper. Series *B* from subject *D* was made with eyes open. Series *E* from subject *E* was made with eyes shut. Series *D* is from subject *W* and was made with carbon paper.

The most striking characteristic of series *A* is the persistent

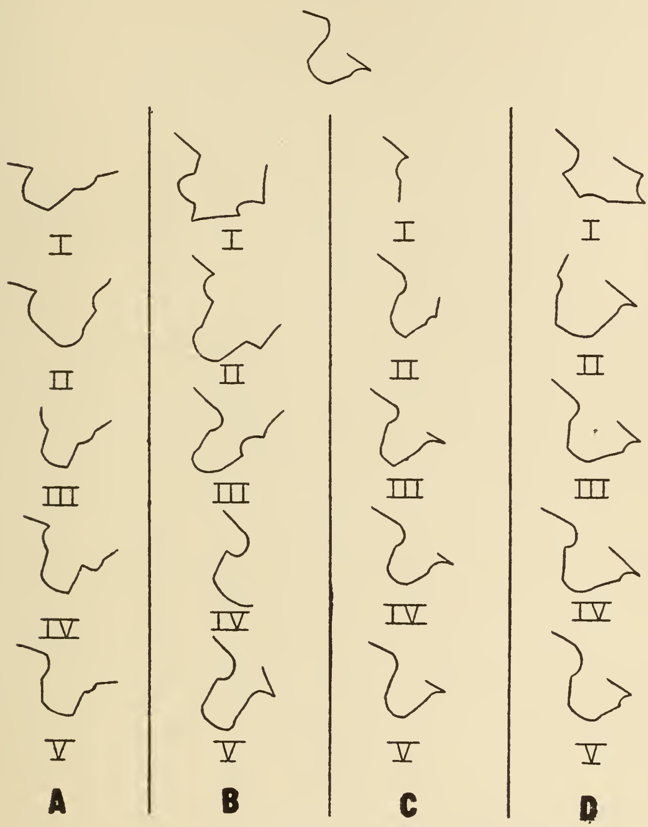


FIG. 95.

error in line seven. It may be remarked that this continued throughout the whole series of ten drawings. As contrasted with the series with closed eyes from this same subject, the carbon paper series seem to show greater distractions.

The striking fact in series *B* is the loss in drawing 4 of the last part of the figure. This is a series with open eyes and yet its results are very much like those shown in *A*, Fig. 94.

Series *C* shows a very typical series for this subject. In no case does this subject attempt to reproduce the whole figure, but always begins with the beginning and gradually masters the figure. The size is better here also than in most of the series.

Series *D* from the same subject as the series *B*, Fig. 94, is reported for the purpose of making it clear that this subject had no such systematic habit of attacking the figures as did subject *E* reported in series *C*. In drawing 2 of series *D*, subject *W* is evidently attending to the last part of the figure and perfecting that before attacking the details of the first part of the figure. Otherwise the series is so much like those discussed that no additional comments are needed.

The analysis of individual series is undoubtedly from many points of view the most productive method of using the results of these tests. It is desirable, however, that the individual results should be compared with results from other subjects and that the same subject should use different figures. For this purpose a system of figures was prepared which should be comparable with one another. The two patterns in Figs. 94 and 95 were members of this system of figures. Each figure was made up of seven lines, four straight and three curved. In constructing the series the relative positions of the lines were varied, otherwise they were repeated without modification in the successive figures, that is, the lengths of the straight lines and the radii of the curves were kept constant throughout the series. With nine such figures six subjects were carried through series of ten drawings. Two of the subjects drew each figure with eyes shut, two drew on carbon paper, and two drew with eyes open.

TABLE I.

Subjects.	Figures.									Total O.	Total S.	Total C.	Total for Subject.
	1	2	3	4	5	6	7	8	9				
E	O–4	S–3	C–2	O–5	S–3	C–4	O–8	S–4	C–5	17	10	11	38
J	O–2	S–3	C–3	O–2	S–4	C–1	O–2	S–4	C–2	6	11	6	23
B	C–1	O–4	S–1	C–5	O–2	S–3	C–3	O–1	S–1	7	5	9	21
H	C–7	O–1	S–5	C–4	O–2	S–3	C–6	O–1	S–3	4	11	17	32
O	S–1	C–10	O–1	S–4	C–1	O–3	S–1	C–2	O–2	6	6	13	25
W	S–4	C–3	O–2	S–2	C–1	O–1	S–1	C–1	O–3	6	7	5	18
Total.	19	24	14	22	13	15	21	13	16	46	50	61	

The most general question which can be asked is, how long does it take for a given subject to master a figure so that he can reproduce it without gross error? In defining what was meant by a gross error, no attention was paid to errors in the length of lines. If a line was omitted, if a curve was in the place of a straight line or the converse, if a convex curve was represented as concave, if a line was 75° or more out of place, the error was regarded as gross. Table I. summarizes the results for six subjects drawing nine figures.

It appears from this table (last column at right) that there are marked individual differences in the different subjects. The difference between *E* and *W* is twenty figures in the total series. Such a difference can not be attributed to any accidental differences in the conditions or in the distribution of figures, it must be interpreted as exhibiting a distinct individual difference. Again comparing *B* with *H*, it is seen that although these subjects worked under the same conditions with the several figures, their results are different and this difference appears in detail also, there being only two cases out of nine in which *B* required more trials to master the figure than *H*. In the second place, the table throws light upon the question of which conditions were most valuable for drawing. When the test was undertaken it was assumed that the drawing would be most accurate when the subjects were allowed to see the results of their work, that is, when they were allowed to keep their eyes open. This expectation was confirmed in general, as shown in the totals under the columns showing the number of trials necessary for *O, S,* and *C* respectively. But there is no great difference between the number of trials necessary to master the figures with the eyes open and with the eyes closed. The carbon drawing is very much more decidedly disadvantageous.

If, now, we turn from the general result to the consideration of individual subjects, we find many cases in which the expectation that the drawing would be most accurate when the figures were seen with the eyes open is not confirmed. Thus subject *E* gives the best results with the eyes shut and very poor results when the eyes are open. The introspections of this

subject throw more light on this result. The observation is repeatedly made that there is confusion because the lines which the subject has drawn disturb the recollection of the pattern. "There is much less confusion when I can consider the figure with my eyes closed and not be disturbed by the results of my own drawing." Again, in such a case as that of subject *B*, *O*, or *W*, it is seen that there is practically no advantage in the opening of the eyes, at least so far as the general mastery of the figure is concerned. There is one respect in which the opening of the eyes is a very distinct advantage. The results presented in the table with regard to the advantages of the different conditions should be somewhat qualified by the fact that no attention is given in this table to the length of the lines. It was true in general that the size of the figure was better when the eyes were open, although the differences here did not seem so essential as the differences which were involved in the mastery of the figure—the matter dealt with in Table I. The result that the drawing was as good in its essential outlines with the eyes closed as with the eyes open makes it clear that the amount of sensory experience received by the subject is not the important matter. It is very much more important that the sensations should be properly arranged with reference to each other in the subject's experience. This comes out very clearly when we consider the fact that the carbon drawing was decidedly disadvantageous. There was in this case an excess of sensory experience because the lines on the defaced carbon paper were somewhat distracting. Furthermore, what sensations there were, in the way of visual experience from the moving hand and muscle sensations from the hand itself, were not given in the usual combination, and consequently confused rather than assisted the subject. The test is, accordingly, very clear as a test showing lack of direct dependence of perception on sensation. Possibly a larger number of experiments would throw more light on this question, for it is obvious from the total results referred to a moment ago that the conditions are in general more advantageous when the eyes are open and least advantageous when there is much confused sensory experience, as in the case of the carbon drawing.

In the third place, the table should be discussed with reference to the relative difficulty of the different figures used as patterns. The totals in the bottom column of Table I. show the number of trials for all of the different subjects necessary to the mastery of any given figure. The experiment was so arranged that two of the subjects in each case used the pattern with the eyes closed, two with the eyes open, and two with the carbon paper. If a sufficient number of cases were present there ought to be a possibility, by the comparison of these different subjects under the different conditions, of determining the relative ease or difficulty of the figures. Nothing very definite comes from the comparison, however, except the general fact that there was not sufficient difference in the different figures to explain the individual differences in the last vertical column, which have been utilized before as the basis of the discussion of individual differences.

There is also no decisive answer, on account of the small number of trials, to the question whether the subjects improved during the nine successive series. The introspections show that some of the subjects learned with regard to the successive figures that they were made up in the same way, and one discovered that the elements were 'probably the same' in all the different figures, but beyond this point there is no clear evidence of overlapping of training. In view of the interest which has been taken in this problem of the transfer of training, it may not be out of place to remark that the discussion is introduced here rather for the sake of calling attention to the danger that there is in appealing to a small number of tests such as here given, for negative conclusions. The whole experiment shows very clearly how complicated the problem is and how many different factors must be considered in even the simple perceptual recognition of these figures. To draw the conclusion, on the basis of the five hundred and forty tests here reported, that there is or is not a transfer of training in successive cases, would be to neglect the whole problem of psychological analysis of the process of learning and to give an empty, formal treatment to the results here tabulated.

TABLE II.

Subjects.	Lines.							Totals
	1	2	3	4	5	6	7	
E	5	13	17	47	63	58	54	257
J	9	19	17	30	56	59	32	222
B	21	30	47	54	43	23	17	235
H	17	36	41	62	58	24	28	266
O	13	24	19	55	49	47	36	243
W	24	21	31	37	41	24	21	199
Totals.	89	143	172	285	310	235	188	

Turning now from the general results reported in Table I. to another matter which can be treated in general, we may inquire what part of the figure is most accurately drawn by the different subjects. In securing a general statement of the errors in the different parts of the figure, a somewhat arbitrary procedure was adopted. Any line was regarded as constituting an error if it was omitted, if it differed from the pattern by 2 cm. or more in length, if its character was changed from a curve to a straight line or the converse, if it was more than 30° from the position given in the pattern. In making the determinations of length and position the curves were dealt with in terms of their chords and not in terms of the arc itself. Table II. shows the number of errors for each of the lines in the ninety trials with each of the subjects. Thus, the horizontal column given opposite the letter *E* indicates that there were five errors in the first line of the ninety drawings for subject *E,* thirteen errors in the second line, and so on. The total number of possible errors was 630 for any given subject. The totals given in the last vertical column show the total error in all of the drawings. The horizontal column at the bottom of the table gives in very striking form the general result. The great majority of subjects drew with relatively great accuracy the first three lines of the figure. There then followed three lines which were always centers of the greatest error; the final line of the figure was again drawn with relatively great accuracy.

Turning from this general result to the records of the various subjects, we find some striking details with regard to the mode of attacking the figure by the different subjects. One of

the most striking cases is that of subject *E*. This subject always began at the beginning of the figure and drew in the first drawing usually three, or at most four lines. The second and third drawings were usually devoted to the mastery of the figure. The results as formulated in Table II. show that there is very little error in this subject's drawing of the first three lines, and very large error for the last four lines, amounting in one case to errors in more than two thirds of the figures. Somewhat similar results appear in the case of subject *J*. Here again it is the first part of the figure which is most accurately drawn. With subject *B*, on the other hand, there is equally clear evidence that the last part of the figure is emphasized. The general procedure on the part of this subject was to draw the figure first with reference to its general form, getting two or three of the lines properly placed and all approximately the right length. The drawing was then refined from the right end toward the left, with the result exhibited in this table that the errors are least in the case of the last line toward the right. Of the other subjects *W* is perhaps the most interesting case. This subject had no definite order of procedure, but corrected in some cases the right end of the figure first and in some cases the left end. The result is that the extremities of the figure and the middle are on the average much nearer to each other than in the case of any of the other subjects. It will be seen from a comparison of the total number of errors here recorded for *W* with the number of trials necessary to master the figure as exhibited for this same subject in Table I., that this subject was superior to the others in ability to draw the figures. Other correlations between the subjects can be made by comparing Tables I. and II. Thus, *H* is high in both cases, although in Table I., *H* is not as high as subject *E*.

Several of the irregularities in Table II. can be accounted for by the fact that the curves were somewhat more difficult to draw correctly than the straight lines. Thus, it will be seen that in the record of subject *O*, the second line, which is a curve, gives notably more errors than the third line, which is a straight line, but which from its position is in general more subject to error. The same fact appears in comparing the second

and third lines for subject *J*. The fourth line, which is also a curve, shows great errors for *O, H,* and *B*. The sixth line, which is a curve, shows large errors for *J*. This difference of error in the curved line should not be attributed to the fact that there is in the series of errors upon which the table was based one type of error which belongs exclusively to the curved lines; namely, confusion between concave and convex curves. This particular error did not occur a sufficient number of times to account for the excesses pointed out in the table.

The figures were in general drawn too large. In no cases were the figures drawn too small throughout a given series except where the eyes were closed. This is shown very strikingly in the case of subject *J.*, in Table III. This table shows for each of the figures the total error in length for each of the first five trials. These errors were measured by rotating over the drawing a small wheel of known diameter which was connected with a reading dial. The total distance traversed by this wheel was then compared with the distance which it traveled in passing over the pattern, and the error in full centimeters is recorded in the table. There is some error in this method of measurement because it is not possible to follow with perfect accuracy the outlines of the figure. The results are presented therefore only in centimeters. Two characteristics, however, are perfectly clear. In the first place, the drawings with the eyes shut were for the most part too small, while all of the other drawings were too large. In the second place, there is greater irregularity in this matter of size than in any other phase of the figure. There is obviously no improvement during the latter part of the test, as shown by the

TABLE III.

SUBJECT J.

Figures.								
O–1	S–2	C–3	O–4	S–5	C–6	O–7	S–8	C–9
8	–5	2	1	1	6	9	0	7
6	–1	4	3	–4	1	7	2	7
4	–2	8	3	–4	5	9	–6	7
11	–2	0	10	–2	5	12	–5	9
9	–4	–1	6	0	2	12	–6	7

excessive errors in figures 7, 8, and 9, unless indeed the large errors in length exhibited in these figures are themselves to be regarded as the expressions of a progressive tendency or product of practice. The error would in this case constitute a kind of negative development. It should be remarked that this table is characteristic of the results for two other subjects. Some of the subjects, however, did not show this tendency to make the figures drawn with closed eyes too small; some of them showed a general tendency to make all of the figures too large.

A general observation which can be made with regard to the drawings of all the subjects is that attention is seldom concentrated at the same time on improvements in the relative angles between the lines and the lengths of the lines. Many of the subjects gave attention in a particular case either to the size or angles, but they did not at the same time attend to both of these phases of the figure. As a means of analysis the successive drawings serve clearly to justify the distinction between the sensory and perceptual processes which are involved in recognizing position and size.

Another distinction which comes out clearly in the results is a distinction between perceptual and ideational processes. Very frequently, as noted above, the subject was quite unable in his introspections to give any definite account of his attention during the introspection of the pattern or the drawing of the figure. In some cases, to be sure, he carried over ideas from one drawing to the next and governed his attention in terms of these ideas. Thus, we find the observation repeatedly made that 'the last drawing was too small,' and in the subsequent drawings we are likely to find some improvement along this line. On the other hand, there are equally numerous cases in which the introspective observation is recorded after the adjustment has been made. The observer evidently noted during the examination of the pattern that his preceding drawing had been too large or too small. The idea came to him in such a case as this while introspecting the pattern and not while he was drawing. Sometimes the idea operated so emphatically that the reaction in the opposite direction could

be noted in the successive drawings. Some effort was made to deal with this relation between the idea which the subject had with regard to his drawing and the efficiency with which he reproduced the figure.

One of the subjects who was drawing a figure too small was allowed to proceed with the experiment beyond the regular ten drawings of the series. On a number of successive days this subject drew from the same figure. Gradually improve-

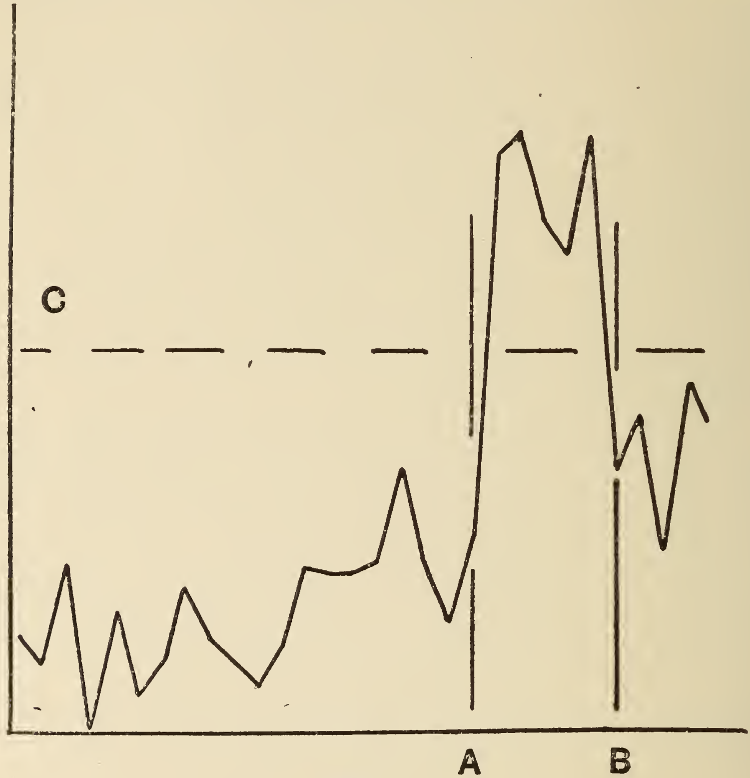


FIG. 96.

ment appeared in his drawings, although these improvements were very irregular in character. At the end of ten series, the subject was allowed to change the conditions of the experiment in such a way that he drew for two days with the results exposed to visual inspection during the drawing. After drawing for two days in this manner he returned to the original

method of drawing with his eyes closed. The results of this series of experiments were presented in Fig. 96 in the form of a curve. The broken line *C* represents the normal length of one of the lines in the figure, this being 6 cm. The line was drawn during the first five trials with a length of approximately half that of the standard. The first point in the curve represents the average of these five drawings. During the next five drawings the length was somewhat further from the standard length than at the beginning. During the third series of five drawings the length of the line was nearer the standard. The curve in Fig. 96 shows that there were a succession of approximations to the standard line. At the point *A* the subject was allowed to give up, as indicated above, the drawing with the eyes closed, and twenty-five tests were introduced with the eyes open. The figure shows a marked error in the positive direction. The line is drawn much too long in these five series. After these twenty-five tests were concluded and a return was made to the series with closed eyes, it will be noted that the effect of the series with the open eyes was carried over only in small part to the new series with closed eyes. There is some improvement over the last drawings with the eyes closed, but on the whole the effect of the interpolated series with the eyes open is not large. It should be stated that not all of the lines in this series of drawings conformed to the curve here exhibited. The one selected line gave a very definite result, the others were more complex.

In concluding the discussion of this series of experiments attention may once more be called to the practical utility of such a series as a means of demonstrating to students the characteristic processes which appear during perceptual development. There is an abundance of illustrative material in such a series of experiments as this which can not be reported in detail in a general paper. Enough has perhaps been reported to indicate the character of the material which can easily be obtained and some of the lines of interpretation which can be suggested to students as the basis for their own critical study of perceptual development as contrasted with ideational development or development of motor habits.

PHOTOGRAPHIC RECORDS OF CONVERGENCE AND DIVERGENCE.

By CHARLES H. JUDD.

This paper reports a series of photographs in which movements of visual convergence and divergence between two fixed points are studied for several positions of the points, namely, in the median plane between the eyes, in certain lateral positions, and especially in the position in which the two points lie directly in the axis of vision of one eye. Five subjects were examined, among them one who was blind in one eye, and consequently exhibited none of the normal forms of binocular convergence and divergence. It is shown by these photographs that there are different tendencies of behavior exhibited by the single eyes of different individuals. There are also differences in the binocular adjustments of different individuals which are probably dependent upon muscular differences in their eyes. All subjects agreed in exhibiting a tendency which is opposed to that of convergent and divergent movements in that they tend to move the two eyes in the same lateral direction. Furthermore, all subjects agree in showing a long and difficult form of adjustment in binocular convergence and divergence. Monocular adjustments are much simpler in type. The report includes certain accounts of voluntary convergence and divergence and concludes with a theoretical discussion in which the theory of coordination of sensory and motor factors is substituted for any analytical description of visual fusion.

The method employed in the investigation to be reported in this paper is in general the same as that reported in the earlier number of this volume of Yale Psychological Studies.[1] By means of a kinetoscope camera the eyes were photographed during movement, a small piece of Chinese white being placed upon the cornea so as to mark clearly in the photographs the exact position of the eye. The double camera there reported, with films which are alternately exposed, was employed throughout, though the figures presented in this article are made up from only one of the films. It is not necessary in reporting the distance and direction of the movements to utilize the results from both films, though in determining certain time relations reference will be made to the double record.

An important modification was introduced in the apparatus

[1] Monograph Supplement No. 29 of the Psychological Review. Yale Psychological Studies, N. S., Vol. I., No. I., pp. 1–16.

in that the camera was driven by a mechanical device. The irregularity which was apparent in the photographic series with even the most carefully trained hand movement was pointed out in the earlier paper. This irregularity has been entirely removed by the mechanical device sketched in Fig. 97. This consists of a large balance wheel *F* which is driven by an electric motor. The heavy balance wheel is necessary in order to maintain uniform motion when the camera is thrown into gearing with the driving apparatus. The balance wheel carries a hollow cone into which a solid cone (*W*) may be firmly set from above. The solid cone is in turn connected with the shaft (*S*) which drives the camera. The camera does not appear in the figure, it stands above the parts here shown. By means of the handle (*H*), which holds the solid cone in a ball-

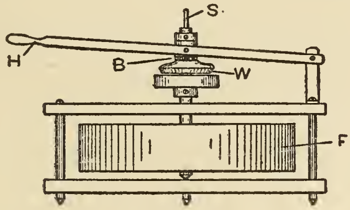


FIG. 97.

bearing collar, the solid cone may be lifted out of the hollow cone, when the shaft (*S*) will be uncoupled from the driving wheel. On the other hand, when the solid cone is set firmly into the hollow cone the shaft (*S*) will immediately be set in operation at the full speed of the driving wheel. The upward and downward movement of the shaft is taken up in a slot device at the upper end of the shaft. This form of driving apparatus makes it possible to begin a series of photographs without any delay such as would be experienced if the driving shaft were set into gradual motion. This is evidently advantageous, for it not only economizes the first and last part of the film while securing photographs at a regular rate of exposure, but it makes it possible to begin at the point of the observer's movements which will be most advantageous, with a certainty that the earliest movements will be fully recorded on the film.

In the earlier reports of the photographic methods atten-

tion was called to the fact that head movements are often present and constitute a matter for consideration in any photographic method of studying eye movements. Two steps were taken in the following investigation: one to throw further light upon this problem of head movement, and the other to eliminate as fully as possible the effects of head movement. In the first place, in order that the exact amount of head movement might be measured, a fixed rod with points of reference quite independent of the head or camera was introduced into the field and photographed with the eye. By reference to this fixed rod it was always possible to determine when a head movement occurred. It was found that head movements occurred most noticeably just before long movements of the eyes. Due to the firm head rest used in our experiments the range of these head movements was never great. No movement was recorded of more than 0.5 mm. If the eyes are looking at an external object which is fixed in its position, there can be very little doubt that there would be required even with a slight movement of half a millimeter, a compensatory eye movement in order to maintain fixation upon the object. These compensatory eye movements would lead to some confusion in the reading of the record. In order to eliminate any possible error from this source, it was possible, and indeed very easy in the investigation here reported, to so arrange that the object should move with the head. The result was that whenever the head moved in any direction the object was carried with it and there was no demand for compensatory eye movement. The objects at which the observers looked in this investigation consisted of two bright points mounted on a bamboo rod. The bamboo rod was held firmly between the teeth and supported at the extreme end on a bar across which it could very easily slide. This use of the teeth to hold the object of fixation involved the abandonment of the ordinary tooth-rest connected with the general head support. The head was supported from the side and back by means of adjustable rods fastened to the seat. Even with the removal of the tooth-rest the head movements, as noted above, were not very conspicuous, while the additional precaution taken in fixing the object to the head was sufficient

to insure absolute freedom from error through head movement.

It may be well in this connection to call attention again to the advantage of one feature of the method which has been employed in all of the photographs taken in the Yale laboratory. The points of reference in all of these experiments have been attached to the head, so that when any head movement takes place, the eyes and the points of reference move together. It is only when the eyes move with reference to the head, therefore, that a record is produced in the photographs. Any method in which the measurements are made with reference to a fixed plate or other point of reference detached from the head will always lead to a confusion of head movements with movements of the eyes within the head. If movements of the eyes are measured with reference to a fixed plate and at the same time the object fixated is detached from the head, then any head movement for which there is at the same time a compensatory eye movement will result in a double error. First, there will be an error because of the movement of the head carrying the eyes into a new position with reference to the plate, and second, there will be an error which arises from a compensatory eye movement executed by the eyes in the maintenance of fixation upon the object of regard. The elimination of any possibility of confusing head movements with movements of the eyes in the head was secured from the very first by the method of attaching the points of reference to the head itself.

In his paper in the Psychological Bulletin of March, 1906, Professor Dodge (page 88) criticizes the use of points of fixation attached to the head on the ground that any head movement must first be measured by certain changes in the relation between the points of fixation, and must then be added as a correction to the records of the eye movements. The real significance of attaching the points of reference to the head seems to have escaped Professor Dodge entirely. The purpose is to prevent the head movement as measured with reference to a fixed plate from being confused with movements of the eyes in the head. These latter movements are the only movements of importance, unless we aim to distinguish between

eye movements which are primary and eye movements which are compensatory. In order to meet Professor Dodge's criticism empirically a number of cases were examined in which there is a distinct record of head movement. The relation of the eyes to the points of fixation is shown in four distinct cases of this kind to be constant in spite of the movement of the head. This furnishes empirical justification for the method in answer to the purely theoretical calculations undertaken in the criticisms referred to.

Before reporting the results of the present series of photographs, it may be well to describe somewhat fully the meaning of the complicated figures which it is necessary to employ. The figures are complex because of the necessity of represent-

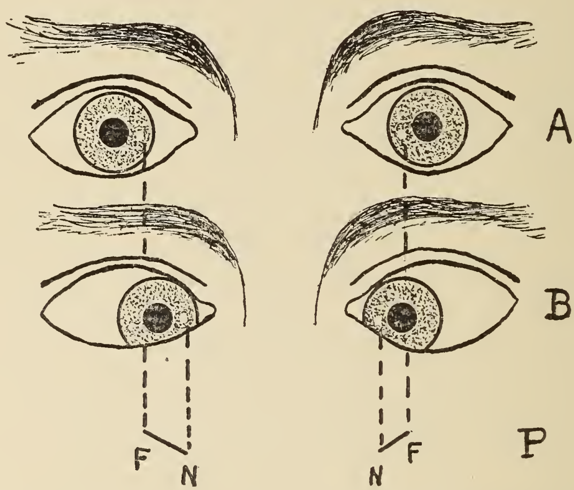


FIG. 98. In the two eyes *A*, fixation is at a point infinitely distant. In *B* the eyes are shown converging on a near point. The convergence here represented is extreme. Th vertical dotted lines show projections of the 'white spot' to the lower part of the figure *P*. The positions *F, F* show the projection of the non-convergent eyes. The downward slope of the lines *FN, FN* indicate the downward movement of each eye in convergence. The positions *N, N* are the projections of the convergent eyes.

ing in a single figure lateral movements and, where they exist, movements upward and downward in the vertical plane. The appearance of the eyes as they are shown in the photographs is schematically illustrated in Fig. 98. The irises are shown in

similar setting of eyelids in two successive positions. These two positions can be projected as shown by the vertical lines and can be very fully represented by the lines *NF* and *FN*. The line on the left indicates that the left eye has moved inward and downward, while the line on the right indicates that the right eye has moved downward and inward. The double movement of the two eyes constitutes a movement of convergence. If the two straight lines are read in the directions *NF* and *NF* they indicate a movement of divergence of the two eyes. Their downward slope indicates that in moving toward the nearer point the eyes not only converge laterally, but also moved downward. If a succession of such movements is to be indicated the successive backward and forward movements can be indicated by a succession of distinct lines placed vertically one under the other.

In the figures reported in this paper the successive lines indicating backward and forward movements are connected by means of dotted lines which are intended merely to show the relation between the end of a given movement and the beginning of the next successive movement. Thus, as will be seen by referring to Fig. 99, photographs 1–10 in the left-hand side of the figure were taken while the left eye remained fixed at a given point. Photograph 11 showed a slight movement of the eye inward toward the nose in a horizontal line. Photograph 12 indicated a further movement in the same direction. The eye remained in this position while exposure 13 was being made. In photograph 14 the eye had reached a still further point of inward movement. At 15 it reached its extreme position of convergence and remained at this point up to and including photograph 21. After photograph 21, the eye made a slow divergent movement back to a position very near to that occupied in photograph 1. This backward or divergent movement is indicated in the full-drawn line 21–22–23–24. The beginning of this line 21–24 is related to the end of the line 1–15 by means of the vertical dotted line which shows the relative positions of the photographs taken during divergence as compared with those taken during convergence. The inward and outward movements represented on the left-hand side of

the figure are for the left eye. They are directly related to the inward and outward movements represented on the right-hand side of the figure for the right eye. It will here be seen that the right eye remains fixed in a given position during the first nine photographs. The right eye began its convergent movement somewhat earlier than the left eye, with the result that it reached the position marked 10 while the left eye was still fixated at its original point. The right eye continued in this position during exposure No. 11. From 11–12 the right eye made the long movement of convergence by which it reached its final position of near fixation. This is indicated in the figure at the right by the completion of the line at 12. The right eye now remains fixed at the point 12 during the whole period of movement in the left eye from 12–15. At 21 the two eyes begin a movement of divergence which proceeds at about the same rate in the two eyes, as indicated by the line 21–22–23–24 on the right. It should be noted again that the photographs in this paper report the results from one film only. If the second film taken with the one reported in Fig. 99 is drawn upon for results, it is found that the movement for the left eye from 11–12 is a slow, continuous movement. This is indicated by the fact that the photograph on the second film which was taken between 11 and 12 indicates that the eye occupied between 11 and 12 a position midway between the points indicated in the figure here presented. The movement from 14–15, on the other hand, is a much more rapid movement. After the exposure 14, there appears on the other film a photograph which shows that the eye moves rapidly to the position 15 and has reached that position before the photograph 15 represented in the figure is taken. In this way it is possible to interpolate between the photographs reported in the figures other photographs which shall increase the precision of time estimations. Thus the average time of the exposures represented in Fig. 99 is 94 σ, while the time calculations can be made by interpolating the second film for periods of 47 σ. This 47 σ can be relied upon in these photographs to be within a probable average error of 2 σ; the time estimations are therefore fairly accurate and the inferences which will be based in

this report on time relations will be restricted to those which are justified by periods of the length here under discussion.

The special problem taken up for investigation in the photographs to be reported in this series is the problem of simple movements of convergence and divergence between two fixed points. These points were placed directly in the median plane between the two eyes or brought into position directly in front of one or the other eye. In some cases the nearer point was somewhat lower than the more remote point, in others the converse relation obtained. The distance of the points was so arranged that the movements of convergence would be clearly marked. No effort was made to vary this distance through any long series of variations. Such additional measurements of different distances as well as other conditions of convergence are very desirable and will ultimately be undertaken. The range of the present investigations was determined by the necessity of solving first of all the definite though relatively simple problem of convergence and divergence within fixed limits. Five subjects were investigated in this series. Mr. Kerrigan and Mr. Cockayne gave a series each, showing the character of their convergent and divergent eye movements when the objects lie in the median plane between the two eyes. Dr. Cameron and the writer were photographed for a variety of different positions and under a number of different conditions. The fifth subject, Mr. G., was an extremely interesting subject for the particular investigation here in hand. Mr. G. is blind in the right eye and has been blind in that eye for a period of about fifteen years. He lost the sight of this eye in an accident when twelve years old. The blindness is due to opaqueness of the lens which was injured by accident. The subject can see vague images and recognizes at times when looking at a bright object that he has double images, especially when his eyes are fatigued. For the most part, however, he neglects entirely the images from the blind eye, these being in any case extremely vague and indefinite. Mr. G. was the subject in several series of photographs which will be reported in full in this paper.

In the course of this investigation it was possible inciden-

tally to secure data on two minor problems which may be disposed of immediately since they are not of importance in the general report. First, in order to secure data on the amount of rotation which occurs during convergence and divergence, one of the subjects was photographed with two white spots on each cornea. When the eye moved in any direction these two spots made it easily possible to determine whether the movement of the whole eye was in the same direction or whether a movement of rotation took place in connection with the general movement of the eye. It was found in a number of cases that when the eye made a long movement of convergence or divergence, rotation factors were involved. In several cases when the eye moved in the convergent direction there was a tendency toward rotation in a clockwise direction through an angle amounting in some cases to 2°. The amount of rotation it will be seen from this statement was slight. Some effort to estimate the probable limits of error in the photographs indicated that the error in reading might amount to 30 min. of arc. Furthermore, it was found that in all cases except one the rotation observed during the movement of convergence was corrected in the later phases of movement as the eye came to its position of final fixation. In the general movement of convergence with which we are concerned in the figures and in this report, rotation plays no appreciable part. The rotation seems to be a phase of movement proper during the adjustment rather than at its termination. Movements of rotation in divergence were also observed in several cases. These movements were both clockwise and anti-clockwise, there being three cases of the latter and two of the former observed in the course of these experiments. The range of these rotations is slightly greater than the range of rotation observed during movements of convergence. It amounted in one case to 3.5°, but was corrected during the last movement by which the eye came to its final position of fixation. This movement of rotation, which was the largest observed in the series, made a difference of less than one half a millimeter in the record as reported in the figures. It was, therefore, well within the probable error of the general observation reported and can be eliminated entirely

from any consideration. The matter of rotation to be productively worked out will require much greater ranges of movement than those with which these results deal.

A second incidental matter to which reference may be made was observed in connection with one series of especially clear photographs in which the eyelid was shown with sufficient clearness in the photographs to be definitely measured in its position. The series of photographs in question involved a downward movement of the eyes during convergence, and it was observed that the downward movement of the eyes was in general accompanied by a downward movement of the eyelids. Indeed, it was clear in two cases that the downward movement of the lids preceded the downward movement of the eye itself. Photograph 42 in one case indicated a downward movement of the lid without any downward movement whatsoever of the eye itself; photograph 43 indicated the downward movement of the eye. The upward movements seemed to be somewhat more irregular in character. In one case a movement of the lid which was evidently a slight movement of winking occurred without any reference whatsoever to the eye movement and at a period when the eye was steadily fixating a single point.

Turning from these incidental matters to the main subject of the report, we may consider first four typical series from different subjects showing the character of the coördination movements when the points of fixation are in or near the median plane and at about the horizontal level of the eyes. Records of this sort are shown in Figs. 99–102. All of these figures show that in converging and diverging upon a point of fixation the two eyes do not in most cases follow paths of the same form nor do they proceed with the same degree of rapidity. The lack of similarity in the motion of the two eyes is here very much more conspicuous than in any of the earlier series of photographs reported in No. 1 of the Yale Studies, where the eyes executed in all series merely lateral movements and did not change their degree of convergence or divergence. It was there noted in a number of instances that the two eyes do not follow exactly the same path or show the same rate of movement, but the incoördination was incidental in that case, in

these results it is a very obvious and primary fact. For example, it will be seen on examining Fig. 99 that the left eye requires for the complete movement of convergence the first time it moves from the remote point to the nearer point a period of about 375 σ, whereas the right eye of the same subject requires

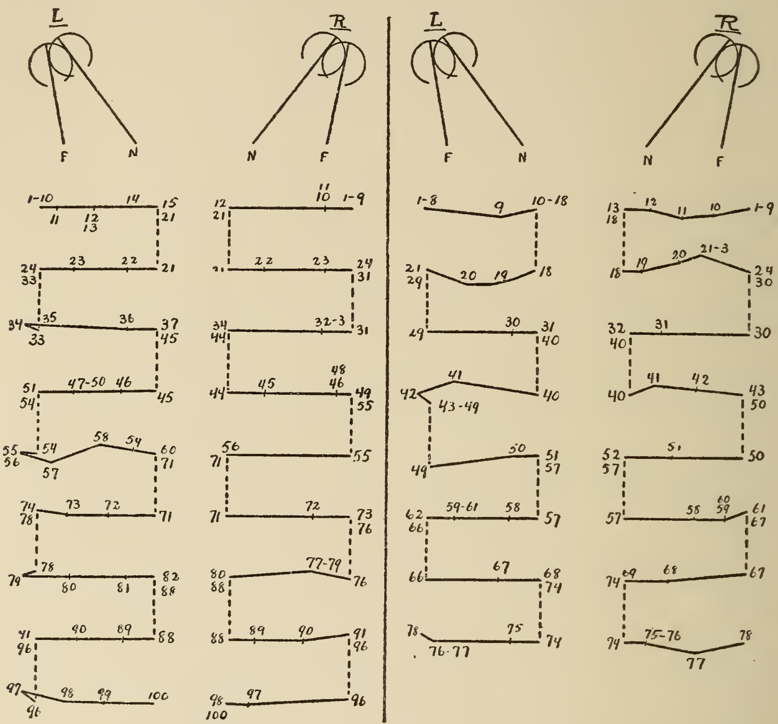


FIG. 99. Subject Mr. Kerrigan. Object of fixation two points, the more remote being at a distance of 55 cm. from the bridge of the nose, the nearer at a distance of 30 cm. The two points lie in the median plane between the two eyes. The average time of the exposure is 94σ. Individual exposures do not depart from this average by more than 3σ.

FIG. 100. Subject C. H. Judd. Points of fixation 30 and 55 cm. respectively from the bridge of the nose. The two points do not lie in the median plane but are slightly foreshortened for the left eye so that movements of the right eye are greater in extent than those for the left eye. The average time of the exposure is 97σ with a departure in individual determinations of not more than 3σ from this average.

for its complete movement from the remote point to the near point somewhat less than 300 σ. The difference is still more notable when we take the third movement of convergence.

The left eye requires from photograph 54, or at least from 56 to 60, that is, four or six photographs, while the right eye in making the corresponding movement requires only the time elapsing between photographs 55–56. The movement of the right eye in this case did not exceed 96 σ, whereas the movement in the left eye certainly did not require less than 375 σ, while if we count the lateral movement from 54–56 the time is nearer 550 σ. Furthermore, the path of the eye movement in the two eyes is shown to be different in form by the lines extending in Fig. 99 from 56 to 60. The same typical difference in the path of movement is seen by comparing the character of the movements of the two eyes between photographs 76 and 82.

This lack of harmony in the two eyes has a special character in each one of the subjects. In Fig. 99 the left eye is in general behind the right eye in all of its adjustments. In Fig. 100, on the other hand, the right eye is slower in its adjustments than the left. The contrast in these two cases is very sharp. In Fig. 101 the right eye is again somewhat more rapid in its adjustments than the left eye, while in Fig. 102 there is no marked superiority in speed of movement in either eye. If we attempt to throw further light upon this individual mode of convergence in which one eye often seems to be decidedly in the lead, we find no evidence which would go to show the relation of eye movements to right-handedness or left-handedness. The subjects of this experiment were right-handed, at least in the case of the three subjects represented in series 100–102. The subject of the photographs represented in Fig. 99 is not now accessible and the question did not arise in time to determine definitely whether or not he is right-handed. At all events, in the case of three subjects, all of whom are right-handed, there is a distinct difference in the behavior of the eyes. In the one case the left eye leads, in another the right, and in the third case there seems to be a balance between the two eyes. Nor is the eye which leads in speed of movement the eye which shows the greater acuteness of vision. The subject in Fig. 100, whose left eye is decidedly in the lead, shows less acuity of vision for the left eye than for the right. The left eye of the

subject represented in Fig. 101 is more acute in vision than the right eye, yet the left eye is slower in its adjustments. According to this very limited evidence, it would seem to be the less acute eye which makes the most rapid adjustments. The true explanation of the relation between left and right eyes is prob-

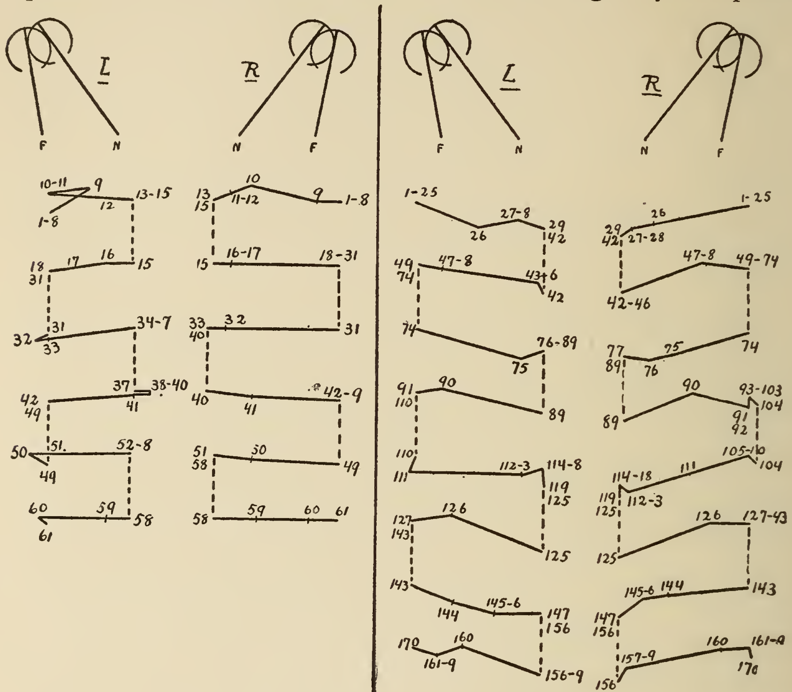


FIG. 101. Subject E. H. Cameron. Points of fixation 30 and 55 cm. respectively from the bridge of the nose. The distance for the left eye is foreshortened. The average time of exposure is 117σ with a possible variation in individual cases of 4σ.

FIG. 102. Subject C. A. Cockyane. Distance of points of fixation 30 and 55 cm. respectively. These points lie in the median plane between the two eyes. The nearer point is somewhat lower than the more remote point with the result that the eyes in converging move downward and in diverging move upward. The average time of exposure is 100σ with a variation not exceeding 3σ in individual cases.

ably to be sought in the fact that there is a difference in muscular balance in the two eyes. The fact that there is a general agreement in character of movement among the four subjects, while at the same time the subjects differ from each other radically in the matter of relation between the two eyes, would

seem to indicate that the relation between the two eyes is a wholly incidental matter. It is not essential to the consideration of binocular adjustment as is shown in Fig. 102, which is a case of almost equal balance of the two eyes. Furthermore, the special characteristics of movement in one eye or the other are more marked in general in the horizontal plane which is represented in Figs. 99–102 than in other planes. Series of photographs were taken with the subjects represented in Figs. 100 and 101, with the points of fixation placed at various angles out of the horizontal. Positions of 40° below the horizontal plane, of 15° below this plane, of 15° and 30° above were taken with each of the subjects. One of these records is reproduced for another purpose in Fig. 114. It is typical in that it shows the same general form of movement as in the horizontal. It may be stated, however, that in general extreme positions above or below tend to induce change in the relations between the eyes. Thus for the subject of Fig. 100 there is a more uniform balancing of the movements in the two eyes than that represented in Fig. 100 for all upward movements. Otherwise the characteristics of the figures are essentially the same. This would seem to indicate as held above that differences in muscular tension are the significant factors in determining the relation of the eyes. When the eyes are turned upward the tensions are not the same as when the eyes move in a horizontal plane. Furthermore, when the balance of tension is entirely changed by bringing the objects into line with one of the eyes, the relation of the eye thus relieved of tensions of its own, to the other eye, is very marked. Cases of this kind will be discussed fully in a later paragraph and are exhibited in Fig. 103. We may sum up this part of the discussion, therefore, in the statement that the two eyes do not move to points of convergence or divergence at the same rate for most subjects nor always in similar paths, but that this lack of similarity in the movements of the two eyes is probably due to external muscular causes and not to internal nervous adjustments.

It is important to notice in connection with the recorded disparity between the movements of the two eyes that fixation is not completed for the subject's inspection until such time as

the slower eye has reached its final fixation upon the point to which attention is given. Evidence on this point was collected in the course of the experiment by giving the subject a reaction key and requiring him to press down upon the key as soon as he was satisfied that he had fixated the point nearer at hand. He continued the pressure on the key until such time as he executed a movement of divergence when he released the key. The record made by pressing and releasing the key was taken through a marker on the same strip of smoked paper as the record showing the rate of the photographs. In this way the subject's introspections with regard to his convergence and divergence were taken in parallel with the photographs showing the position of the eyes. Referring to Fig. 101, for example, the subject's reaction record shows that he was satisfied that he was looking at the near point when photograph 14 was taken. He was satisfied again that he was looking at the more remote point at photograph 20. The following introspections show the near point at photograph 34; remote point photograph 44; near point again at photograph 52; remote point at photograph 63. In all of these cases it will be observed that the reaction is slower than the final adjustment of convergence or divergence by enough to allow in each case for the reaction time of the hand after fixation had been completed. In no case does the subject make the introspective reaction before the movement of convergence or divergence of both eyes has been completed. This is typical of the introspections of all the subjects. Records of introspections were taken with the great majority of the records upon which this paper is based and there is no case in which the introspective record does not bear out this general statement.

A second general characteristic of all the movements shown in Figs. 99–100, and the other similar records mentioned above, appears in the fact that the time of convergent and divergent adjustment is relatively very long. For comparative results which do not involve convergence and divergence, reference may be made to certain photographs that will be reported in the present series and are represented in Figs. 117 and 118 of this paper. It will here be seen that the first two movements

in Fig. 117 required certainly not more than 180 σ each, and the second movement which lies between photographs 12 and 13 could not have required more than 90 σ. Similar evidence can be drawn from many of the earlier series reported in the first number of the Studies. The movements of convergence and divergence as contrasted with these simple lateral movements required a period of time which is in general 350 σ or more. Furthermore, the movements of convergence and divergence here represented show themselves to be complex and difficult in the frequent pauses which are made by the one eye or the other in passing from one point of fixation to the other. It is obvious that we have to deal here with a complex form of adjustment. This general fact can not be explained as could the lack of uniformity in the movement of the two eyes, by any reference to the external muscular structure of the eyes. The eyes are converged or diverged in such a way as to fixate definite points, and these movements involve a careful adjustment with many pauses and corrective movements which consume the long period of time required for this adjustment. The evidence is not wanting that the movements are in many cases movements of fine adjustment after the main movement has been executed. For example, in Fig. 102 movements of the left eye between 28 and 29, between 75 and 76, between 113 and 114 are all of them obviously in the nature of final corrective movements. Other examples of similar character can be seen in all of the figures. This final adjustment of the eye would seem to signify that the complete execution of a movement of convergence or divergence is in the nature of a slow and careful adjustment of the eye to a stimulus which is in some form or other recognized as not completely met by the main movements. We shall call attention in the later discussion to other evidences which go to show that the movement of convergence is under the constant direction of the stimulus and is a movement of fine and difficult adjustment.

A third fact which may be noted in certain of these figures now under discussion, but which will be very much more fully illustrated in later figures, is the fact that in some cases the eyes before they begin the careful adjustment of convergence, exe-

cute a lateral movement in which the two eyes sympathize by moving in the same direction rather than in opposite directions as required for convergence or divergence. A conspicuous illustration of this is to be found in Fig. 101 in the movement between photographs 31 and 32. After photograph 31 the right eye moves in a long sweep toward the left, thus reaching a position required for convergence. The left eye, on the other hand, instead of moving in such a way as to converge upon the nearer point, executes like the right eye a movement from right to left. This movement is directly opposed to the movement of convergence which must later be executed by this eye and it is explicable only by assuming that the left eye sympathizes automatically in its behavior with the stronger impulse of the right eye. A similar fact may be seen in Fig. 99 in the movement which lies between photographs 96 and 97. Here again the right eye executes a long movement toward the left and the left eye follows in the same direction. The tendency for the two eyes to move in the same lateral direction at the same time, rather than in the direction of convergence or divergence, seems, therefore, even in these earlier photographs to be sufficiently strong to assert itself with all clearness.

Stronger evidence of the fundamental character of this tendency to move the two eyes in the same direction comes out in a number of series of photographs taken with the objects of fixation so arranged that they lay directly in the axis of vision of one of the eyes. Two series of photographs of this kind are shown in Figs. 103 and 104. These photographs are from the subject reported also in Fig. 101. In Fig. 103 the left eye is obliged to make a movement toward the right whenever it changes its center of fixation from the remote object to the nearer object. The right eye, on the other hand, is not called upon to change its line of regard in looking from the remote object to the near object, it simply changes the degree of accommodation of the lens when it has finally reached its position of rest. It is a very striking fact that the process of adjustment in the right eye which is called upon to maintain a single line of regard, is quite as complex as the process of adjustment in the left eye which is required to make large movements of con-

vergence and divergence. Indeed, in some respects it is even more complex, and this complexity can be described very simply by the statement that whenever the left eye makes a movement of convergence or divergence there is a tendency for the right eye to follow the same path of movement as the left eye. Thus, when the left eye moves toward the right between photo-

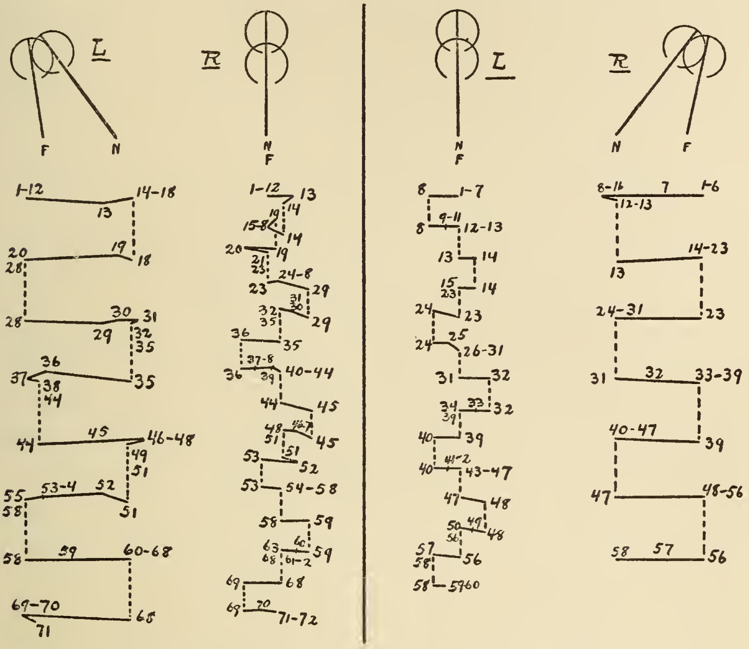


FIG. 103. Subject E. H. Cameron. The objects of fixation at 30 and 55 cm. respectively from the bridge of the nose. These points lie in the line directly in front of the right eye so that the right eye in fixating the two points is not required to move its axis laterally. The average time of exposure is 120σ with a possible deviation in individual cases of 4σ.

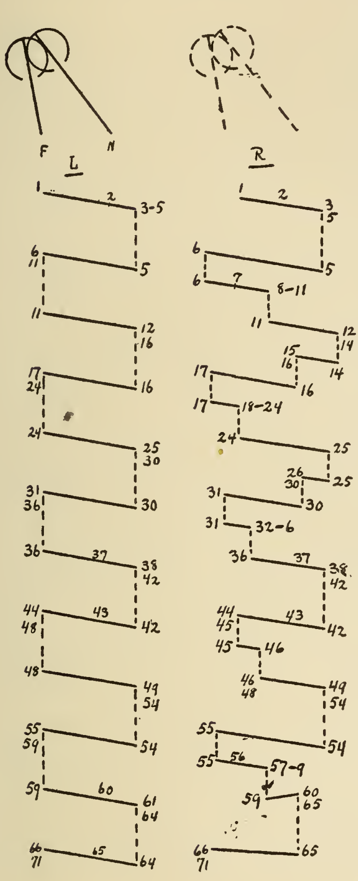
FIG. 104. Subject and conditions like those in Fig. 103 except that the two points of fixation here lie in the line directly in front of the left eye. Average time of exposure 124σ with a possible deviation in individual cases of 4σ.

graphs 12 and 13 the right eye also makes a movement toward the right. This movement of the right eye brings it out of line with the two points of fixation. There follow, therefore, a series of movements of readjustment, as indicated in the figure, until in photograph 15 a final fixation is reached satisfactory to the subject. It is interesting to note in this connection that the introspection of the subject showed that he regarded

the fixation as complete only after photograph 15. Continuing in the detailed examination of Fig. 103, we find that after fixating the near point for a period of time the two eyes between photographs 18 and 19 execute a slight movement of divergence. This movement of divergence showed itself in both eyes. This and other evidences throughout the figures seem to show that divergence is simpler than convergence. But even divergence gives way before the strong tendency to complete sympathy in lateral movement. In the case in hand, divergence is evidently only preliminary to the further adjustment, for there follow from 19 to 20 two like or sympathetic movements in the two eyes, both of them moving toward the extreme left. The right eye is involved by this sympathetic movement in the necessity of a secondary readjustment which is not fully accomplished until photograph 21. The introspective record shows that the subject's reaction for divergence parallels the 23d photograph. Between 28 and 29 there is another sympathetic movement between the two eyes. The readjustments which take place from 29 to 32 evidently involve a different type of the sympathetic relation between the two eyes. The right eye is involved in the necessity of moving from 31 to 32 in order to regain the natural position of fixation upon the near point. The left eye seems also to have overshot the mark of fixation between 30 and 31. It is consequently involved in a movement of readjustment. Another case of this kind of correction for the left eye seems to appear between 48 and 49.

It will be remembered that the subject of these photographs in Fig. 103 showed a natural tendency, as shown in Fig. 99, to make more rapid and independent movements with the right eye than with the left. The strong sympathetic behavior of the right eye with the left, as reported in this series of photographs in Fig. 103, is a clear indication of the fundamental and natural character of the sympathetic movement of the two eyes. The fact of sympathetic movement appears in the reverse relation in the photographs reported in Fig. 104. Here, again, the eye which is not called upon by the conditions of the experiment to execute any lateral movements, that is, in this case the

left eye is constantly involved in sympathetic adjustments in which it moves at first in the same direction as the right eye and then afterwards by a series of readjustments comes back



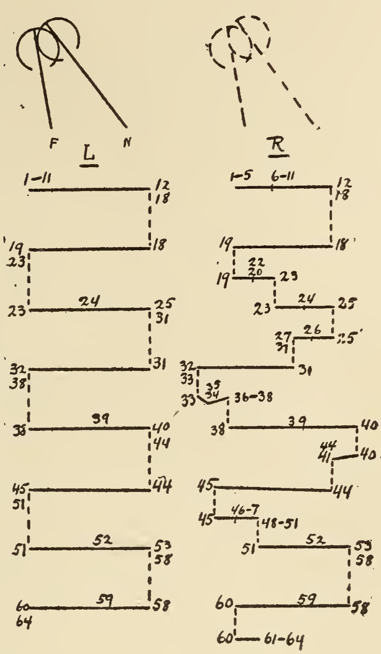


FIG. 105. Subject Mr. G. who has been for a period of years blind in the right eye. Points of fixation 50 and 25 cm. respectively in the median plane between the two eyes. The near point somewhat lower than the more remote point. Average time of exposure 104σ with a possible deviation in individual cases of 3σ.

FIG. 106. Subject same as in Fig. 105. Points of fixation 50 and 25 cm. respectively from the bridge of the nose in the primary horizontal plane. Average time of exposure 82σ with a possible deviation in individual cases of 3σ.

to the original point of fixation. Series of exactly the same type as here obtained were secured from the subject reported

in Fig. 100. The general statement here worked out in detail is also supported by earlier observations. Professor Dodge in an earlier paper on eye movements[1] referred to facts of the same sort as here described.

Further evidence of the fundamental character of this sympathetic movement of the two eyes was obtained from subject G., who, as was noted above, has been for a long period blind in one eye. Figs. 105 and 106 represent two series of photographs from this subject. It will be seen from the examination of these figures that the right eye, which is blind, does not make movements of convergence or divergence as its primary movements in any case. The first movement of the right eye always consists in a distinct and extensive movement in the same direction as the left eye. For example, between photographs 11 and 12 the left eye makes a long movement from the far point to the near point. The right eye makes a movement of about the same extent in the same direction. Not only is the lateral movement in this case in the same direction, but the movement of the right eye is downward in keeping with the movement of the left eye. After a short interval the right eye, as seen in the line between photographs 14 and 15, changes its position in such a way as to come back somewhat toward its original position in photograph 11. This it will be noticed is not a movement of convergence in any proper sense of the word, it is rather a tendency to readjust the position of the right eye so that it shall keep its original position before it made a movement in sympathy with the left eye. In like manner, when the left eye makes a movement of adjustment from the near point to the more remote point, as indicated in the line 16–17, the blind eye makes a similar movement. It later corrects this long sympathetic movement between 16 and 17 by a short movement of readjustment between 17 and 18, but here again the position 18 is not the same as the original position 16 from which it started. The final position of the right eye at any given moment seems to be more or less the accidental resultant of the accumulated tendencies of earlier sympathetic movements. The movements of the blind eye are, however,

[1] *American Journal of Physiology,* 1903, VIII., p. 328.

very clearly sympathetic movements and not movements of an independent character. The same facts appear in Fig. 106, where the blind eye shows even a more marked tendency to sympathize with the left eye. The left eye in this case executes a somewhat longer movement than in Fig. 105 and the result appears in the very long sympathetic movements in the blind eye. This is notably true between photographs 31 and 32, between photographs 38 and 40, and between photographs 44 and 45.

It is important to note in connection with these photographs of subject G. that the whole activity of convergence and divergence is reduced to a very much simpler level when it is a monocular adjustment. This is evidenced not only by the photographs exhibited in the figures, but also by the introspective testimony which was taken in parallel with the photographs. Thus, the subject reacted for complete convergence in parallel with photograph 4. Divergence was shown in parallel with photograph 7. The next convergence falls upon photograph 12, divergence on 18, convergence on 26, divergence on 33, convergence on 38, and so on through the series. It thus appears that the convergence and divergence of the subject who sees with one eye only are free from the delays which are required in the case of subjects who see binocularly, and the form of movement is at the same time very much simpler in character. There seems to be, therefore, clear evidence not only that the lateral movement is the simpler form of united action of the two eyes, but that when lateral movement is the only form of movement required the whole adjustment is simple in its character.

Further evidence with regard to the simplicity of mere lateral movement is to be derived by an examination of monocular convergence and divergence, as shown by normal subjects when a single eye has been covered up. Figs. 107 and 108 show two simple cases of convergence and divergence after covering one eye. These photographs are from the subject reported in Fig. 100, and a comparison of these figures with the results in Fig. 100 show immediately the much simpler character of the movements. The time required for the monocular adjust-

ments is relatively shorter. For example, in one case between photographs 37 and 38, the whole movement is executed in a period of time considerably less than 100 σ. By referring to the second film it was found that the eye had reached the position indicated in the figure by 38 at the time when the photograph was being taken which lies between 37 and 38. In other words, the total movement from 37 to 38 as a movement involved between 45 and 90 σ. The adjustment between 49

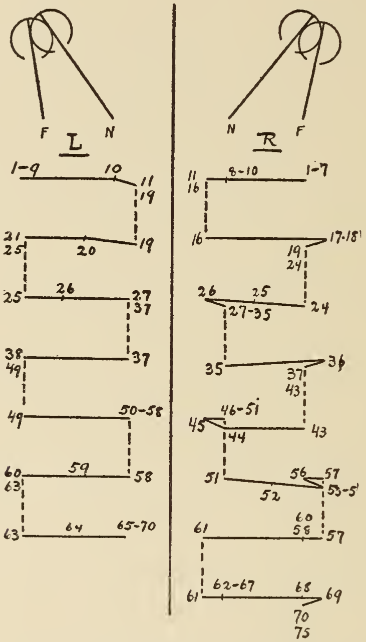


FIG. 107. Subject C. H. Judd. Points of fixation 55 and 30 cm. respectively from the eye and in a line directly in front of the nose. The right eye is entirely covered. All movements recorded in the figure are accordingly for the left eye. The average time of exposure is 94σ with a variation not exceeding 3σ for individual exposures.

FIG. 108. Subject and conditions same as for Fig. 107 with the exception that the left eye is covered and movements here recorded are for the right eye. Average time of exposure 90σ with a possible deviation of 2σ from the average.

and 50 was also sufficiently rapid so that its period can be described as distinctly below 100 σ. The introspective records of the subject show that complete divergence and convergence was secured in these two cases in parallel with the next succeeding photograph, as in most of the earlier records. There

is, therefore, obviously no delay such as is demanded in binocular convergence and divergence.

The movements of the eye in Fig. 108 show certain distinct complexities in character. These complexities may be described by the statement that when the eye is moving in a given direction there is a tendency for it to move too far in the direction in which it is traveling. Thus, between 16 and 17 in Fig. 108 there seems to be so long a movement that there is necessity for a readjustment as indicated between 18 and 19. Similar facts are shown in the readjustments between 26 and 27, between 36 and 37, between 45 and 46, between 69 and 70, and a very complicated readjustment appears between 55 and 57. The meaning of these readjustments in monocular convergence and divergence is not easy to make out. Evidently the right eye in this subject is less regular in its behavior than the left eye. This may be related to the fact that the right eye shows greater irregularity of movement in the earlier series in which both eyes are involved. The right eye in this subject, as stated above, is the eye which shows the greatest acuity of vision. It is, therefore, improbable that its less precise adjustments at the end of the movement are due to retinal conditions. It is probably true that the muscular balance of the two eyes is not the same and it is not unlikely that the pecularities of behavior are connected with these muscular inequalities rather than with the internal or retinal processes.

Collateral evidence in view of this conclusion can be offered in the case of the subject of these photographs by means of experiments discussed by the writer in a paper published in *Science,* 1898, VII., pp. 269–271. The experiments in question may be tried by covering one eye and steadily fixating a bright object with the uncovered eye. The covered eye should now be suddenly uncovered when there will appear double images which are crossed and therefore indicate that the center upon which the lines of regard of the two eyes were converged was more remote than the object fixated by the open eye. The position assumed by the eye which has been covered in this case will undoubtedly depend in part upon its own tendencies of relaxation. That the center of convergence is further than

the point of fixation would indicate that the natural tendency of relaxation in the covered eye is in the direction of a movement of divergence. The distance of the double images from each other at the moment of uncovering the eye will give some clue to the degree of divergent tendency in the covered eye. The observation of this double image requires some practice. The writer has made long series of observations with his two eyes and finds that the distance between the after-images when the right eye is covered is greater than the distance when the left eye is covered. The muscular tensions of the right eye seem from this experiment, therefore, to be somewhat more pronounced than the muscular tensions of the left eye. This result is clearly in agreement with the typical differences in the behavior of the two eyes, as shown in the photographs exhibited in Figs. 107 and 108. That the right eye should be for this subject more difficult to bring into binocular convergence and divergence, as is shown in Fig. 100, seems also to be clearly in agreement with these evidences regarding the monocular behavior of the right eye. Such considerations show the difference between binocular and monocular adjustments and at the same time show also their intimate interrelation. Monocular peculiarities constitute the negative tendencies which must be overcome in order that binocular adjustments may be accomplished. Binocular adjustment is therefore a complex process in which monocular tendencies and tendencies toward lateral sympathetic movements must be replaced by a more elaborate form of coördination.

The simplicity of adjustment in monocular vision can be further attested by means of the series of photographs from another subject, as reported in Figs. 109 and 110. In these two series the camera was set in motion before the eye which was not to be used in the full series was covered. The result is that the first movements are binocular. The binocular movements here reported did not continue long enough to give very full evidence, but the second binocular movement in Fig. 109 agrees with all the other records from this subject, as for example, those reported in Fig. 101. In considering the first movements in Figs. 109 and 110 it should be recognized that the first

movement often differs from the later movements. At all events there can be no ambiguity regarding the character of the monocular movements of this subject. Though they consist of adjustments to near and remote points they are ex-

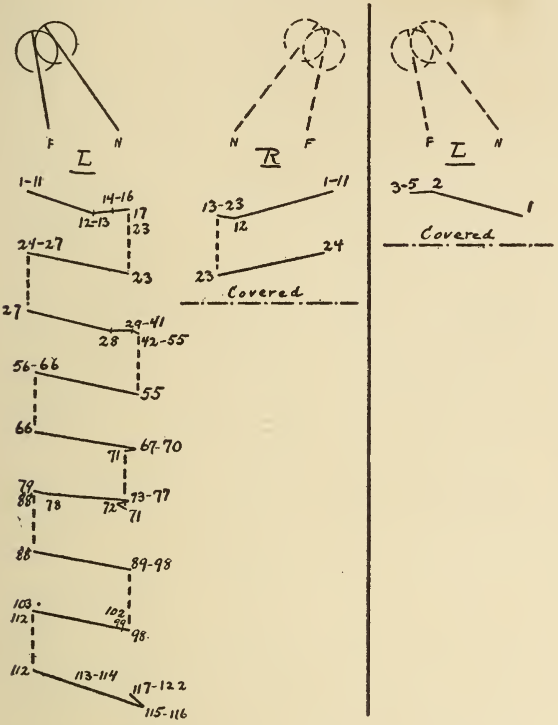


FIG. 109. Subject E. H. Cameron. Points of fixation 55 and 30 cm. respectively in the median plane between the two eyes. The axes of vision are directed upward in looking at the more remote point and somewhat lower in looking at the near point. After twenty-six exposures the right eye was covered so that the remainder of the record shows the movement of the left eye only. The average time of exposures 75σ with a possible deviation in individual cases of 2σ.

FIG. 110. Subject and conditions the same as Fig. 109 with the exception that the left eye was covered in this case as indicated in the figure after five exposures. Average time of exposures 75σ with a possible deviation in individual cases of 2σ.

tremely simple and in striking contrast with the binocular adjustments of the same subject as reported in Figs. 101 and 114.

One special case of monocular adjustment which is of crucial importance for experimentation with monocular vision may

be investigated by the methods here employed. If two points are placed in a direct line in front of one eye, and the second eye is covered so as to be entirely excluded from participation in vision so far as retinal factors are concerned, it is found that the open eye is involved in certain adjustments which suggest

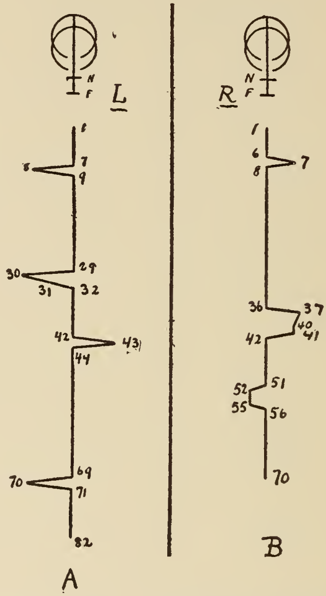


FIG. 111. Subject C. H. Judd. This figure represents the eye movements by a different method from that adopted in the earlier figures. The vertical length of the line indicates the number of exposures. Thus, in *A* the distance from the beginning of the line to the movement which is indicated at 7 is on the same scale as the distance from 9 to 29 which represents a continuous fixation of the eye at one point during 21 exposures. Slight movements of the eye about the point of fixation are neglected in this figure. *A* indicates the form of motion for the left eye, *B* the form of motion for the right eye when the two points of fixation lie in the direct line of the visual axis of the open eye, the right eye being covered while the photographs were being taken for the left eye, and the left eye being covered while the photographs were being taken for the right eye. The average time of exposure for both *A* and *B* was 96σ with a possible variation in individual cases of 2σ.

the continued participation of the covered eye in the movement of convergence and divergence. The conditions here under discussion are related to those which are presented in Figs. 103 and 104. It was there found that the eye which was not required to make any lateral movement in fixating near and remote

points did, nevertheless, by virtue of its sympathetic relation with the eye that was involved in elaborate lateral movements, execute certain unnecessary sympathetic movements and certain supplementary movements of readjustment. In Fig. 111 two series of photographs are reported which deal with this case which has often been assumed by experimenters to be a case of pure monocular vision. In the series of photographs represented in *A* the right eye was entirely covered and the points for fixation were so arranged as to lie directly in front of the left eye. The introspections of the subject show that at a point corresponding to photograph 10 the subject was satisfied that he had fixated the nearer point. The fixation of the nearer point called for no movement whatsoever of the left eye. The two movements which lie between 7, 8 and 9 must, therefore, be explained as a sympathetic movement and a movement of readjustment. The right eye which was in this case entirely covered up undoubtedly made some movement. That the covered eye does not cease its movements in binocular adjustments can easily be demonstrated by laying the finger lightly on the lid of a closed eye and observing the movement of the closed eye when the open eye changes its fixation from a remote to a near point or the converse. The normal subject who makes this observation will discover that the closed eye tends to converge and diverge not as in the case of the blind subject reported in Figs. 105 and 106, but rather in a way similar to that reported in Figs. 103 and 104. In the case in hand the covered right eye undoubtedly executed a movement from right to left while the left eye was making its movement in the same direction from 7 to 8. The left eye then made a rapid adjustment from 8 to 9, while the right eye continued its movement of convergence.

A similar case appears in the figure in the movement and adjustment between 29 and 32. Here the subject's introspections show that the satisfactory fixation of the near point coincides with photograph 32. A sympathetic movement of divergence with the corresponding introspective evidence appears between 42 and 44. These photographs show with all clearness that the covered eye is by no means eliminated from con-

sideration when an adjustment from a remote to a near point is under discussion. These facts constitute a fatal objection to the methods and results which have become classic in psychology for experimentation on monocular accommodation. Among recent investigators, Arrer, Hillebrandt, Baird and others have placed the objects of fixation exactly as they were placed in this series of photographs in a direct line in front of the open eye. They have covered or closed the other eye and proceeded with their investigation on the assumption that they had eliminated binocular influences, at least in major part. All that they have eliminated in such cases are the retinal images involved. There is a very large influence, as shown in these photographs, so far as the movements of convergence and divergence are concerned. Furthermore, the effects on the behavior of the open eye are by no means uniform either in character or in mode of occurrence. For example, it is seen in Fig. 111 by contrasting the behavior of the right and left eyes of the same subject under similar conditions, first, that the movements of the left eye are more extensive than those of the right eye, while adjustments are in general very much more rapid. The readjustments in the case of the right eye are so slow that in some cases, as for example between 36 and 42, a period comparable to the period ordinarily required for convergence is involved. That different subjects would probably differ in the extent to which the closed eye influenced the open eye is indirectly evidenced by the fact that the blind subject in whom one eye had long been disused made very irregular movements with what in his case corresponded to the covered eye. In his case the blind eye was almost completely dominated by the tendencies of movement in the normal eye. It is probably true in certain subjects that one eye is much more independent than in others. There may be individuals for whom the control of normal vision is sufficiently strong to dominate the whole movement. Indeed, there were in the case reported in Fig. 111, between photograph 8 and photograph 36, three movements, two of divergence and one of convergence, for which there is absolutely no evidence of any readjustment in the open eye. This itself is sufficient evidence that the influ-

ence of the covered eye is somewhat irregular in character. In some cases apparently the open eye dominates, in others there is a clear domination of the covered eye. The net result is that the binocular influence not only is not withdrawn, but it is complicated in an unknown fashion by the various tendencies

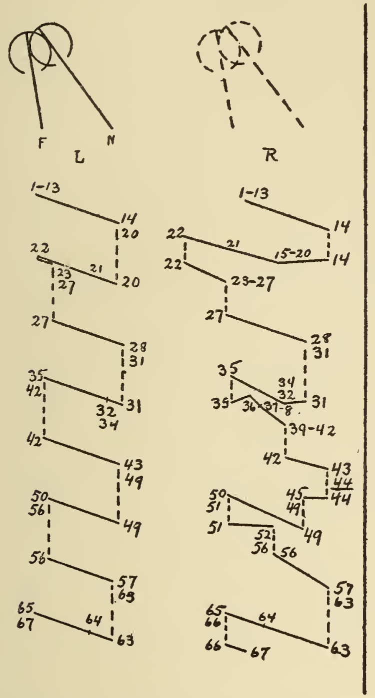


FIG. 112. Subject same as in Fig. 105. Distance of points from the eye 50 and 25 cm. respectively. Points placed in a line directly in front of the right eye such that if the eye were capable of vision the points would lie in the line of fixation of the right eye. Average time of exposures 73σ with a possible deviation in individual cases of 3σ.

FIG. 113. Subject as in Fig. 105. Points of fixation 50 and 25 cm. respectively in front of the bridge of the nose. These points were placed in the line of fixation of the left eye. Average time of exposures 76σ with a possible deviation in individual cases of 3σ.

which produce in the open eye movements which are wholly uncalled for by the demands made upon that eye considered by itself. The reason why no conclusive evidence as to the character of monocular vision has ever been derived from the experiments of Arrer, Hillebrandt and others becomes per-

fectly obvious in the presence of these facts. If any method of experimentation with monocular vision is ever to be devised it must be recognized as a fundamental fact that monocular vision is not produced by simply covering one eye.

In order to test this matter with the blind subject, the conditions were so arranged in the series of photographs reported in Figs. 112 and 113 that in the one case the points of fixation were placed in a direct line in front of the blind eye, in the second case the points of fixation were placed in a direct line in front of the normal eye. Unfortunately in both cases the nearer point was placed somewhat lower down than the more remote point, with the result that an adjustment upward and downward was involved in the change of fixation between the points. The overwhelming importance of the normal eye is clearly attested in Fig. 112. There is one interesting case in which movement 22–23 seems to show some lingering influence of the blind eye. In moving from 21 to 22 there seems to be an excessive movement in the case of both eyes. The recovery in the same direction in both eyes seems to follow in 22 to 23. Whether this is a mere accident of adjustment in the left eye or a direct case of relationship of movement in the two eyes is, of course, impossible to say; it is certainly not a typical fact, for it does not appear in the other parts of the series.

In Fig. 113 there is exhibited up to photograph 30 a close sympathy between the two eyes. There is one exception to this sympathy, namely, between 11 and 12, where the blind eye moves downward at the same time that the normal eye moves upward. There is also a readjustment in the normal eye between 25 and 26, after which there seems to be a great uniformity in the behavior of the normal eye. This uniformity in behavior of the normal eye follows upon a series of readjustments prior to photograph 26, which readjustment may indicate that the subject was selecting the method of fixating the objects at which he was to look. At all events there is a difference between the adjustments above photograph 26 and below. There is in the series of photographs for the blind eye also a radical readjustment between 30 and 34. This readjustment seems to have no related fact in the behavior of

the normal eye. Furthermore, the activity of the blind eye never becomes, as does the activity of the normal eye, regular in direction and extent. There is the greatest possible irregularity exhibited in the case of the blind eye. If these photographs justify any conclusion with regard to normal vision, those conclusions seem to be in support of the general conten-

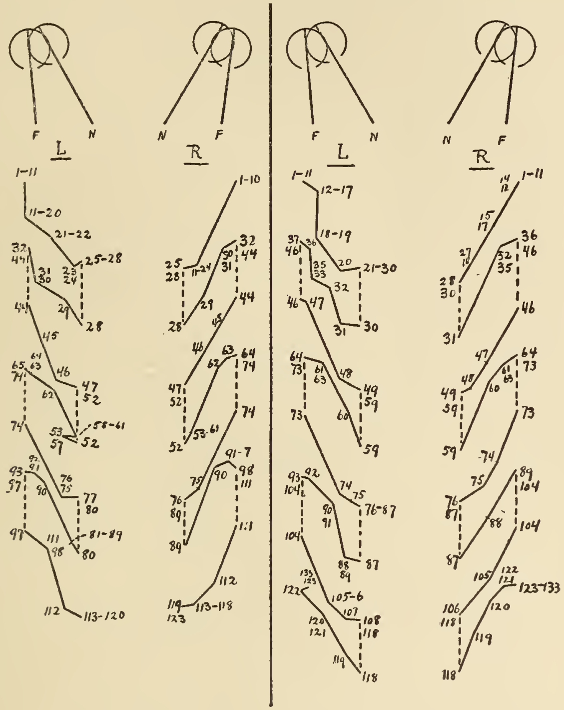


FIG. 114. Subject E. H. Cameron. Points of fixation 55 and 30 cm. from the bridge of the nose, the more remote point being much higher than the nearer point. Average time of exposures 73σ with a possible deviation in individual cases of 2σ.

FIG. 115. Subject and conditions the same as in Fig. 114. The series of photographs here reported were taken one year and three months later than the photographs reported in Fig. . The average time is 75σ with a possible deviation in individual cases of 2σ.

tion that the eye which is not guided in its behavior by retinal images is extremely irregular in its activities. So far as the criticism of the methods of monocular experimentation are concerned this figure supports the general contention that the influence of the closed eye is extremely irregular.

Returning from the discussion of monocular movements to other cases of binocular adjustment, Figs. 114 and 115 report in detail the results of two series of photographs in which the convergence on the near point was accompanied by a very pronounced downward movement of the eyes. It will be seen by examining these figures that the adjustments were fairly well balanced on the two sides in point of time, although in general as in the earlier series for this subject the right eye is somewhat more rapid in its adjustments than the left. This appears in Fig. 114 in the movement between 74 and 77 and in Fig. 115 in the movements between 87 and 93, and 104 and 108. There are, however, one or two cases in which the left eye seems to be somewhat in advance. The series are reported here not so much for the sake of reiterating the statements made in the series reported in Fig. 101, as rather for the sake of calling attention to certain characteristics of the eye movements which show the difficulty of securing photographs of monocular movements that are significant without reference to the behavior of the other eye even when both eyes are open. There is a very obvious case in Fig. 114, between 89 and 98, and another in Fig. 115, between 59 and 64, in which divergent movements of the two eyes are made up of two essentially different components: first, of a general oblique component, and second, of a horizontal component which must be explained as a corrective movement to effect a more complete divergence than was attained in the oblique movements. There are a number of other cases in the figures here reported where the eye movement is of the same general type on one side but not on the other. Thus, in Fig. 114 the movement of the right eye between 52 and 64 is of the form under discussion, whereas the corresponding movement for the left eye is of a much more complicated character. The same is true in Fig. 115 when we compare the movements of the right eye between 31 and 36 and the corresponding movements of the left eye. Furthermore, it will be seen by considering the movements of convergence reported in Figs. 114 and 115 that there is a reversal in the form of movement in that the horizontal component of the movement occurs at the bottom of the line

rather than at the upper end. Thus, it will be seen in Fig. 114 that the movement between 74 and 77 is a direct reversal in all essential respects of the movement which immediately follows it between 89 and 98. The movement of the left eye between 44 and 47 in Fig. 114 is of the same type. In Fig. 115 the movement between 73 and 76 and again the movement between 104 and 108 show the typical form of a final sharp change in the direction of the general movement. When these final adjustments in the movement are related to the whole system of binocular adjustments rather than considered merely as forms of curvature of the eye movement, it will immediately be seen that they constitute finer corrective adjustments of the eye by which it fixates the final point upon which both eyes are to converge or diverge. In other words, the form of the movement is a form of adjustment not a type of natural muscular direction of monocular movement. When the final point of fixation is in the main above or below, there will be a general movement of the eye upward or downward and a finer adjustment which is more nearly in a horizontal plane. We may regard the upward or downward components of these movements as simple sympathetic forms of movement. Since there is a strong tendency for the two eyes to move together they do not reach immediately their respective positions of fixation, but make an error which is always explicable by the principle of sympathetic movement of the two eyes.

Furthermore, the results exhibited in Figs. 114 and 115 show certain cases in which the simple adjustment described above did not appear. Indeed, there were cases to which we may call attention in which the curvature was in the opposite direction. For example, a clear case of irregular movement appears in Fig. 114 in the case of the left eye between photographs 28 and 32. The corresponding movement for the right eye conforms very clearly to the type described above as typical, but the movements of the other eye correspond more nearly to the movement which would ordinarily appear in convergent movements. In Fig. 115 the same general irregularity is to be observed in the movements of the left eye between photographs 11 and 21 and also between photographs 30 and 37. A slight

irregularity which throws the whole curve into some confusion is also to be found in Fig. 115 for the left eye, between 87 and 93. Furthermore, there are in the two figures two movements for the right eye which are as nearly straight as possible. In Fig. 114 there is a straight movement between 44 and 47. In Fig. 115 the same form of movement appears between photograph 11 and photograph 28. Indeed, attention may be called to the fact in this figure as in the earlier figures for the same subject that all of the movements of the right eye seem to show a somewhat greater regularity and precision than do the movements for the left eye. We may, therefore, conclude on the basis of these figures that all monocular movements are merely exhibitions of the complex adjustments necessary in bringing the eyes into harmonious fixation. It may be added that the figure here reported for one subject is entirely like in essential character figures obtained from two other subjects also.

A final series of facts to be reported were obtained by photographing the eyes during voluntary convergence and divergence in the effort to fuse stereoscopically two separate points without the aid of any stereoscopic apparatus. Two points were drawn on a plane sheet of paper and the subject was asked to look first at one point with both eyes and then at the other, and finally by voluntary convergence to so cross the optical axes that the left eye should fixate the right-hand point and the right eye should fixate the left-hand point. The result would be the familiar fusion of double images in such a way that three points would be seen; one resulting from the fusion of the two figures in the two eyes and the other two from the monocular effects of the two points in the two eyes.

For this experiment two subjects were available, one of whom, the writer, has long been familiar with this form of adjustment of the eyes and performs it readily and without strain, the other subject is unable to make the adjustment readily. After a good deal of effort he is at times able to bring about the fusion for brief intervals, but it is likely to be lost immediately and it can be secured only by strenuous effort. Fig. 116 reports the results for the subject who is not easily able to make the adjustment. The form of the figure is some-

what different from that which has been used in the earlier figures except in Fig. 111. A continuous vertical line such as that between 5 and 20 indicates that the eye continued for the whole period to fixate a single point. From 20 to 21 both eyes move together toward the left, from 22 to 23 both eyes move together toward the right and so on. It will be noticed by

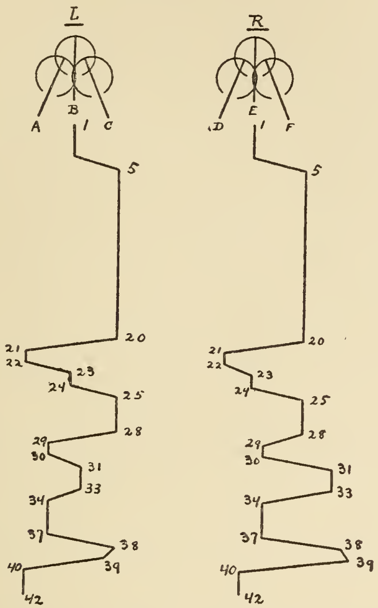


FIG. 116. Subject E. H. Cameron. Two points at a distance of 10 cm. from each other placed at a distance of 45 cm. from the eye are to be fused by a voluntary crossing of the optical axes of the two eyes such that the left eye will fixate the point on the right and the right eye will fixate the point on the left. The successive efforts of the subject to accomplish this voluntary fixation are reported in the figure, the vertical dimensions of the figure being such that distances between exposures are represented by proportionate lengths of vertical lines. Average time of exposures 70σ with a possible deviation in individual cases of 2σ.

considering the length of these various movements that the left eye moves, for example, in the movement 20 to 21 through a greater extent than does the right eye. Conversely, between 30 and 31 the left eye moves much less than does the right eye. There is only one point in the whole series at which the two eyes differ from each other in the direction of their movement, that is, between the photographs 38 and 39, and this is

immediately corrected by a movement of the right eye between 39 and 40 much greater in its extent than the corresponding movement of the left eye. The whole figure shows clearly the difficulty experienced by the subject in attempting to bring about any voluntary convergence of the two eyes. He evidently was dominated throughout these movements by the natural tendency to move both eyes in the same horizontal direction. There is some evidence from the position of the successive points of fixation that he attempted to find a point somewhere between the two objective points given him, and this effort to find an intermediate point is doubtless to be interpreted as his effort to bring about some adjustment which should be a compromise between the two leading tendencies to fixate the right and left points respectively. There is some promise, in the fact that the two eyes do not make the same length of movement, that he would ultimately attain the adjustment necessary to bring about the fusion of the double images. Unfortunately the series of photographs was not continued long enough to record this result, but the accidental separation of the two eyes by moving one more than the other is evidently a suitable means of bringing about the final fusion of the points. This doubtless explains why the subject in question is unable to hold the point steadily for any long period of time.

Fig. 117 represents a series of movements of voluntary convergence by the writer who, as pointed out a moment ago, is capable of making this movement without serious difficulty. Between 5 and 7 the two eyes move from the point at the right to the point at the left. Between 12 and 13 the two eyes come back again to the original point of fixation. The next movement is the first effort at voluntary convergence and it will be noticed that there is a marked sympathetic behavior, both eyes moving toward the left between 15 and 16. The right eye now continues its movement toward the left until it succeeds in fixating the point at the left. This is accomplished in photograph 21. The left eye, on the other hand, by a succession of movements between 16 and 22 gradually comes back to the position which it originally held at 15. The left eye thus returns to the fixation of the right-hand point. Indeed, there

was no reason so far as the physical relations of the fovea and the right-hand spot were concerned for any movement whatsoever of the left eye when the voluntary convergence began. There was, however, in keeping with all of the facts which have been reported in the earlier cases, a definite sympathetic tendency between the two eyes such that the left eye departed from its fixation of the right point in sympathy with the right eye and was then obliged to make a series of corrective adjustments until it should again fixate the right point. From 22 to 28 the eyes were held in voluntary convergence in the crossed position attained by the preceding movements. From 28 to 29 there was a return to the fixation of the point at the right-hand side. Here, again, it will be observed that there is a strong sympathetic tendency between the two eyes. Indeed, the behavior is exactly analogous to that which was described in connection with the series reported in Fig. 104. In Fig. 104 the left eye was, so far as the objective relations were concerned, under no necessity of movement. It was, therefore, more than in any other case dominated by the sympathetic impulse to move with the right eye, which was objectively under the necessity of long movements. The rest of Fig. 117 will be easily understood after the descriptions which have been given. One notices, furthermore, that the tendency toward sympathetic action decreases in extent as the subject increases in experience with this particular adjustment. It is a familiar observation to anyone who has performed these movements of voluntary convergence that the successive efforts to fuse two given objects are less and less difficult until fatigue sets in. The photographs in this series were not continued long enough to show any marked effects of fatigue.

Fig. 118 represents a series of efforts on the part of the subject to fuse two points by divergent movements. For this purpose the two points must be somewhat nearer than the two points in the earlier experiment in voluntary convergence. Indeed, the two points upon which the subject is to voluntarily diverge the optical axes can not be further apart than the pupils of the two eyes. The whole figure was in this case held somewhat nearer to the face, with the result that the amplitude of

the movement from one point to the other is approximately the same as the amplitude of movement in the case of voluntary convergence. The movements of voluntary divergence

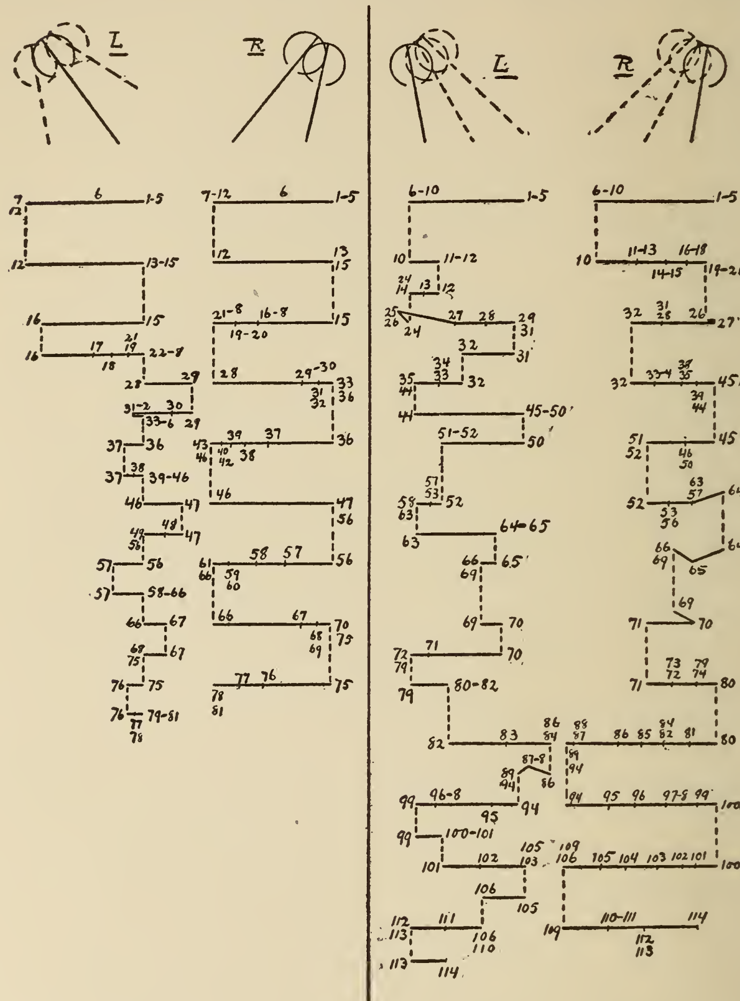


FIG. 117. Subject C. H. Judd. Showing voluntary convergence such as to fuse two points 10 cm. apart, situated at a distance of 50 cm. The first two movements reported in this figure are lateral movements from one point to the other without any effort at convergence. Average time of exposure 89σ with a possible deviation in individual cases of 3σ.

FIG. 118. Voluntary divergence in the effort to fuse two points held at a distance of 35 cm. and separated by a distance of 5.5 cm. Average time of exposure 92σ with a possible deviation in individual cases of 3σ. Subject C. H. Judd.

are always more difficult for the writer than movements of voluntary convergence. This statement hardly needs to be made in view of the clear evidence to this effect which appears in the series of photographs. The final position of the eyes in this case when fusion has been attained should be for the right eye the position occupied in photograph 5, or at least approximately in this general position, while the left eye should correspondingly occupy a position of fixation corresponding to that held in photograph 4. It will be observed that in 14, 35, 58, 72, 99 and 112 the left eye is in such a position as to indicate fixation of the left-hand point. The right eye is in a position which indicates fixation of the right-hand point at 27, 45, 64, 80, 100. Obviously the center of fixation for the right eye must be regarded for practical purposes of fusion as lying somewhat further to the left than these particular positions which have just been recorded, for the eye does not continue at 27, 45, etc., for a sufficiently long period to give the opportunity for definite fusion. Positions 19, 39, 57, 97 and 114 are maintained for a longer period and would seem to constitute the positions of relatively permanent fixation. There is obviously also some tendency for the left eye to fixate permanently the region which lies somewhat to the right of the positions corresponding to position 6. It would, therefore, seem to be true that in voluntary divergence the optical axes are not brought into a position of direct fixation upon the point, but the eyes are so adjusted that the points to be fused fall approximately at the centers of vision, though in reality somewhat at one side. This confirms the general results reported throughout the earlier investigations of eye movements both in the Yale Studies and in the paper of Dr. Dearborne,[1] where it is shown that there is no definite center of fixation, but a somewhat extended area which is entirely satisfactory to a subject.

There are a number of other characteristics in Fig. 118 which are worthy of special mention. Numerous evidences of sympathetic tendencies of movement in the two eyes are apparent. Thus, between 10 and 11 both eyes move toward the right. The left eye then returns to its original point of fixa-

[1] *Columbia Archives of Psychology,* No. 4, 1906.

tion between 12 and 14, while the right eye by a series of stages comes to the fixation of the right-hand point at 19. Again, between 50 and 51 both eyes move toward the left between 52 and 58; the left eye continues in this movement toward the left, while the right eye by a series of stages returns to a position of fixation at the right in photograph 57.

There are in the later part of the figure two curious examples of the difficulty sometimes observed in making movements of voluntary divergence. It will be seen that between 82 and 84 both eyes converge very notably. The same is true between 100 and 106. The movement of voluntary convergence, as indicated above, is easier and more fully developed than the movements for voluntary divergence. Consequently, when two points are presented to the subject there is a tendency at times to execute the movement of convergence even when the movement of divergence is intended. Indeed, it is frequently true that when the observer intends to secure fusion by voluntary divergence he secures fusion which ultimately proves to be fusion by convergence, although he may be for the moment quite unconscious of the failure to diverge and the actual fact of convergence. This reversal of the movement may also be connected with the fact that the eyes were becoming somewhat fatigued after the earlier series of efforts at voluntary divergence.

It remains in referring to the photographs to call attention to one or two general characteristics which appear in a number of the different plates. The first few adjustments of the eyes are very frequently different in character from the later adjustments. This has already been pointed out in connection with Fig. 117, where the sympathetic movement was very marked between photographs 15 and 16. In Fig. 115 it will be observed that the movements toward the end are more regular in character than those at the beginning. In series 113 a very radical change in the character of the movements appears. In Fig. 101 a good illustration appears of a very radical readjustment at the beginning of the movement of the left eye. These instances tend to confirm the statement made in the first reports of this method of photographing the eyes, that it is very desir-

able for the movements of the eyes to be recorded through more than one adjustment. A single adjustment is very likely to consist in a mere preparation for the later activities, which often become very much more regular in character after the first preliminary adjustments.

The second general fact is that in all of these adjustments there is a tendency for one or the other eye to depart either for a single photograph or for a short series from the direct line of movement. This is illustrated in Fig. 118 at photograph 64 and again in photographs 87 and 88. The same general tendency appears in the case of subject G in Fig. 112, photograph 36. Photograph 26 in Fig. 110 is another illustration of the same type. Photograph 36 in Fig. 103, in Fig. 102 photographs 47, 90 and 126 for the right eye show these same general characteristics. These departures of the eyes from the definite line of movement, especially in a vertical direction, are probably to be explained as due to muscular tensions in the superior and inferior muscles. They are not essentially different in character from the slight readjustments necessary in the lateral directions; they appear, however, somewhat more irregular because the main tendency of movement in all of these cases is in the horizontal rather than in the vertical directions.

The relation between the eye movements reported in this paper and the processes of perception to which these eye movements are related can not be defined in any simple formula of sensations of movement. It is perfectly evident that the binocular fusion of figures which is attained through the completion of movements of convergence and divergence is a process which involves much readjustment of natural tendencies toward sympathetic lateral movements. The evidence has been presented in sufficient fullness to make it clear that there are certain natural individual tendencies of movement in the eyes of different persons, and certain forms of behavior simpler than convergence and divergence, which tend to creep in during any series of fixations of near and remote objects. These irregularities of eye movement, as we may very properly call them from the point of view of completed convergence, are

seldom if ever presented to normal consciousness and certainly do not constitute positive contributions to the perceptual process. Indeed, it is necessary for us to assume that perception consists in some process which overcomes these irregularities in convergence. There must be some guiding motive which brings together the two eyes in spite of thcir tendencies to follow forms of movement which are simpler than those required for successful convergence.

This superior motive which stands above eye movements and controls them is certainly not explicit intention on the part of the observer to direct the eyes in their action, nor is it an explicit recognition of any irregularities after these irregular movements have been executed. One can by careful attention to his visual experiences become more or less clearly conscious of the fact that an elaborate adjustment is required for perfect recognition of objects upon which the gaze is fixated. Professor Dodge has made the very keen observation that there is always a period of what he calls ' clearing up ' whenever a visual object is fixated. A little practice makes it possible for any one to observe this period of ' clearing up,' especially if the visual adjustment is from a remote to a near object. The recognition of such a clearing up period is as explicit a perceptual recognition of irregularities in eye movement as one is likely to attain. What the clearing up process actually consists in or what are the irregularities in eye movement which precede it no one can describe through his introspective observations. This is evidenced by the fact that though a great variety of observers have attempted to deal with the problem of binocular adjustment in movements of convergence and divergence, their observations are relatively incomplete as compared with the photographic evidence which can be secured in objective study of these adjustments.

The lack of explicit consciousness of eye movements that shows itself in lack of recognition of the irregularities in these movements which are present in obedience to the sympathetic tendencies of the two eyes, argues for a lack of direct relationship between visual perception and sensations of eye movements. It may be said by the defenders of the eye movement

theory that it shows merely the impossibility of analyzing movement sensations out of the total complex of visual perception. The sensations are present, these defenders will hold, and are of cardinal importance in determining the recognition of position, but their importance is not in their own specific quality and intensity, but rather in their relation to the other factors involved. In answering such a statement it can be pointed out that many of the phases of movement are in direct opposition to the whole process of convergent and divergent fixation. The eye movements not only are not analyzed out of the total situation, but many of them are in direct opposition to the character of adjustment which the subject is aiming to attain. Furthermore, they are not only in opposition to the binocular adjustment, but they are of a distinctly lower order than the binocular adjustment itself. These evidences go to show, in spite of all that has been written with regard to the sensations that result from these movements and especially with regard to the tensions toward movements which are frequently regarded by the defenders of the movement sensation theory as of greater importance than the movements themselves, that the movements are overcome and redirected rather than relied upon to accomplish the adjustment.

We can not, however, be completely satisfied with mere negation. When we have accumulated the evidence to show that there can not be a direct relation between the sensations derived from irregular eye movements and the final completed percept, we must, on the other hand, recognize that there is the closest possible relation between the final adjustment and the complex percept of position in depth of the object fixated. In other words, the perceptual process and the movement are ultimately brought into harmony, though it is obvious that this harmony is obtained through effort and involved many irregular and conflicting factors. That there should be an ultimately successful binocular adjustment, in spite of the conflicting tendencies shown in the results above described, is the strongest argument for the recognition of a relation of fundamental importance between perception and movement. Furthermore, there can be no doubt in the light of the facts described in this

paper that the binocular adjustments differ in complexity and in the degree of effort necessary to attain them from certain simpler forms of adjustment.

In attempting to explain the complex forms of binocular adjustment we must recognize the fact that the adjustments themselves are in response to some demand which is of superior importance in individual life to the elementary tendencies which they overcome. The natural temptation will immediately arise to dispense with the whole question here involved by saying that the binocular motor adjustments are controlled by the retinal images. This will serve very well as a short formula if we wish to discuss merely the relation between sensations of movement and retinal sensations. It is undoubtedly true that the retinal sensation is in a certain sense the controlling factor in the total binocular fusion. It is only when like stimuli act upon the two foveas that the eyes can come to rest and the converging of the lines of regard as a condition preliminary to holding the two eyes fixed upon a certain point in spite of their natural tendency to move together in a lateral direction, is undoubtedly related to the superior significance in experience of the retinal sensation from the two foveas.

The short formula is, however, incomplete until some clearness can be reached as to the nervous organization by which the retinal excitations are related to movement. Sherrington[1] has made it clear that the retinal excitations are sources of independent central processes. He has further called attention in his book *The Integrative Action of the Nervous System*, pp. 385, *et seq.*, to the fact that the fusion of these independent central sensory processes is due to their union as they pass to the motor centers. Experiments on central localization have shown in agreement with the functional fact reported in this paper, that the movements in the two eyes which result from the stimulations of the occipital visual area are of the type described above as sympathetic lateral movements; that is, whenever this first cortical area in the occipital region is stimulated there is a tendency for the two eyes to move laterally in

[1] Sherrington, 'On Binocular Flicker and the Correlation of Activity of Corresponding Retinal Points,' *British Journal of Psych.*, Jan., 1904, pp. 26–60.

the same direction. It is only when one of the higher association centers situated further forward in the cerebrum is aroused to action that there is any tendency toward convergent movement of the two eyes. Convergent movement is thus obviously dependent upon a higher form of associative activity in the central nervous system than that which is demanded for mere lateral movement of the two eyes in sympathetic activity. Thus we see that while the short formula which holds that retinal images guide the adjustment of convergence and divergence is satisfactory for the statement of the contrast between sensations of movement and retinal sensations, it is by no means acceptable as a complete statement of the whole formula of fusion. Fusion in this case evidently involves a coördination of impulses of an elaborate associative type. If that coördination is of the simple type provided for in the first visual region of the cortex, the motor adjustment will be of the more elementary form. If, on the other hand, convergent coördination is present, this appears as a higher type of coördination than that which is provided in the simpler sympathetic adjustment, and for the more elaborate coördination higher centers must be drawn into action and correspondingly a higher form of experience must be expected.

We are thus led from our considerations of the relation between retinal images and movement process to recognize the fact that after eliminating sensations of movement we must recognize the paramount importance of motor adjustments in an entirely different sense. The important question is one of coördination of impressions in the development of more and more elaborate forms of response. It is not a question of how fully we are informed through sensations of the adjustments after they are made, it is rather a question of how elaborately we are able to respond to sensory impulses through higher forms of associative coördination. Experience will thus be explained not so much by the factors which enter into it as by the forms of elaboration to which these factors may contribute. The movement is not significant because of its peripheral elements which, as this report has shown, involve an oscillation backward and forward with a resultant tendency

toward gradual convergence or divergence. The movement is rather significant because as a total form of activity it is worked out as a response to certain complicated sensation processes which are themselves unified and fused in experience in the development of the unitary motor adjustment. The unity of experience is not to be described by regarding the one group of sensation factors as in control and the other elements as clustered about the dominant sensations. The whole experience is to be regarded rather as a succession of associative processes in which the end of the process may very properly be described as a motor adjustment to all of the sensory elements. The stages of that process may be variously complicated by monocular tendencies and sympathetic tendencies in one direction or the other. But these secondary or complicated elements are all of them significant merely as indications of earlier stages of coördination and fusion, not as contributing any positive factors to the present fusion. Indeed, there are many evidences that the preliminary motor adjustments are not important for the final coördination, but mere incomplete and partial phases of the total process. Thus, a consideration of the time required to correct one of the irregular movements which appears whenever one eye is distracted from the path of convergence in order to sympathize with the other eye is so short that there is no possibility of clear perceptual consciousness intervening between the execution of the movement and its correction. There seems to be a momentary oscillation between phases of adjustment. The first tendency is for the two eyes to move sympathetically in a given direction. Before this tendency can be consummated in any such way as to come to clear consciousness and invite a voluntary change in the direction of one of the eyes, a second phase of movement sets in which obeys the demands of higher coördination. Taken by itself, each phase of sympathetic and corrective movement seems to be an involuntary or reflex adjustment of the eye. On the other hand, when we consider the total process of conscious perception and motor coördination, we recognize that the essence of the process is not in the single factors but in the combination. As we saw in Fig. 117, the mastery of the sym-

pathetic form of movement in the course of a series of convergent and divergent fixations becomes more complete as the series of adjustments proceeds. It would seem, therefore, that the clearer and clearer perception of a given situation results in a more and more affective subordination of the discordant adjustments to the demands of the total adjustment. The total process is thus shown to be a higher form of coördination than any of its elements and to stand in a very different relation to conscious experience.

If for any reason the coördination is delayed, the separate factors may assert themselves. Thus, in the adjustment indicated for Fig. 116, the most characteristic fact in the subject's experience is the utter confusion of his visual percepts. It is quite impossible for him to guide his visual activities because the single factors assert themselves in such a way as to prevent final coördinated activity. Again, the observation that prior to the final adjustment there is a clearing up period in experience is a fact of the same type. These cases of incoördination or confusion form the most productive starting point for an explanation of the nature of perceptual fusion. Whenever one is confronted by a mass of experiences for which he has no definite mode of response, that mass of experiences is confusing just because it distracts him in such a way as to attract a great variety of conflicting adjustments. He tends to move now in one direction, now in the other, without succeeding in performing completely any single reaction. We may say of his movement on the one hand that it is uncoördinated. We may say of his experience on the other that it is entirely lacking in unity and in clearness. Nor will the unity and clearness of experience and the corresponding coördination of behavior result by any mere adding together the different elements of sensory experience or motor response. The fusion must be worked out by bringing about such a relation between the different phases of adjustment that they shall all be included and combined into a new form of coördinated completeness which shall have a unity of its own type, a unity superior to that of the elementary forms of sensation and activity which it embraces.

Such a formula as this is applicable in the discussion of all

kinds of perceptual development. There can be very little doubt that an infant finds itself overwhelmed with a great mass of sensory experiences by which it is constantly distracted. The few forms of adjustment which are clearly worked out through instinct in the infant are the only clear-cut forms of adjustment which it has and its earliest attention is evidently directed toward the objects to which these instinctive coördinations apply. Undeveloped adult percepts certainly illustrate the formula of incoördination of factors. Thus, if one looks at a new and complex visual pattern he fails to recognize clearly the complex figure because the mass of lines is so distracting that each draws his attention away from the other. Both in the case of the infant and the confused adult the surest method of attaining a clear percept of any given phase of his environment is to gradually work out a practical adjustment in which activity and sensory experience shall be unified by subordination of most of the elements to a few dominating factors. That is, instead of making a succession of reflex responses to a great variety of different factors, the subject must ultimately reject some of these reflex tendencies. If they are so strong that they tend to assert themselves in spite of the dominating center upon which he would converge, he must then work out a higher form of coördinative adjustment whereby he shall be able to recover from the strong reflex tendency which has led him astray. He must, in short, by some means or other succeed in overcoming the great manifold of distracting experiences. When he has thus succeeded in withdrawing from many avenues of impulsive adjustment, he may ultimately work out a form of adjustment which will be of a much more unitary and stable order. He will then find that there has been going on in his sensory experience a process of selection whereby certain factors have been distinctly subordinated to a few dominant elements of experience.

The dominant elements in such a process can not now be described as having asserted themselves through the complete suppression of the other elements; nothing is suppressed in the sense of being eliminated. The sensory processes can not be shut out, and the energy poured into the central nervous system

by the minor sensation can not be ignored. The selection of the particular centers of experience and adjustment is rather a matter of relations in which all factors are included. The adjustment instead of being merely negative includes the total individual. The concentration is not merely a matter of sensory elements, it is not merely a matter of muscular adjustments; it is a matter of general relating and placing of factors in advantageous relations.

There has been in the discussion of perceptual fusion in psychology some tendency to confuse two distinct characteristics of percepts which characteristics have, so far as their conditions are concerned, entirely different types of origin. First, every percept has the characteristic of inclusiveness, and in the second place every percept has unity. The inclusiveness of a percept is ordinarily much larger than the descriptive analysis of percepts by introspection would admit. One does not ordinarily recognize, for example, in the binocular adjustment which we have been studying, the natural tendency to recognize a great variety of objects in the lateral regions of vision, although these apparently ignored objects undoubtedly enter into the determination of eye movements. One does not recognize through any introspective analysis the fact that when a sympathetic movement is performed by the two eyes there is an increase in the confusion of retinal images sufficient to call for an immediate readjustment. One does not recognize explicitly, as even the most ardent defenders of movement sensation theories are prepared to admit, the sensations of movement which come from the muscles of the eye or the sensations of contact which come from the surroundings of the eyeball during its adjustments. Indeed, if these various factors which are unquestionably contributory to the total percept came into any clear recognition, they would constitute a disturbance of the perceptual process. They are included but are not recognizable factors. They are fused with the chief elements which in this case consist of the retinal elements, but their fusion is of such character as to subordinate them to the main sensations. The main sensation would have no perceptual setting if it were not for these surrounding sensation factors.

Inclusiveness thus emphasizes the great variety of factors entering into percepts. In sharpest contrast with the inclusiveness of percepts stands their unity. The more factors included in a percept the more obvious the demand that they shall be reduced to a single coördinated system. Unity must be sought elsewhere than in the manifold of sensory factors. Unity is, accordingly, wrought out of experience by counteracting the tendencies that grow out of wide inclusiveness. Inclusiveness may be regarded as the primary character of consciousness, and in so far as fusion is used to describe the inclusiveness of a conscious state we may say that inclusiveness is the most elementary fact in mental life. It is certainly more elementary than concentration on single clearly defined experiences. A single clearly defined experience is a late product of perceptual development. It comes as a result of the narrowing down of inclusiveness; it does not in any sense of the word arise by any process of mere addition of factors to each other nor by a mere ignoring of factors.

The notion of addition or subtraction should be eliminated altogether from the discussion of perceptual fusion. The only concept which is of any value in the clear explanation of perceptual unity is the concept of coördination. This concept is one which gives us the justification for treating the processes of perceptual fusion as processes of unification, and unification, as will be seen from the foregoing discussion, is totally different from inclusiveness. When one succeeds in building up in his experience a compact percept, he has not succeeded in doing this by bringing into consciousness factors which were not there at the outset, nor has he attained it by bringing together factors which formerly existed apart. He has done it in most cases by utilizing certain adjustments which were all present at the same time but mutually incompatible, and he has developed a form of adjustment which can in some measure reconcile the incompatible factors. Consider, for example, in terms of the material presented in the earlier part of this paper the mass of retinal, muscular and tactual sensations which are involved in any act of binocular convergence or divergence. Consider, on the other hand, the clearness with which con-

sciousness moves to its perceptual goal in spite of all of the elements of experience. So far as this binocular adjustment of convergence is to be described in terms of inclusiveness, it comprises a great number of distracting and unnecessary elements. When, on the other hand, we recognize how the adjustment of the two eyes overcomes all of the distractions that precede it, we see how closely related are the final motor adjustments of the eyes to our clarified and definite spatial organizations of experience. We bring all the data of sensory experience together and recognize in one act of consciousness the distance between a remote point and a near point at the same time that we accomplish a careful adjustment of the two eyes upon the near point. Both processes involve a subordination of many of the elements of the environment which are constantly impressing the retina and many of the muscular adjustments which arise from the collateral and unnecessary tendencies of eye movement. This perceptual experience is, however, a compact unitary process which has a certain definite content about which all of the other elements are related, and it derives through its unity and through the compactness of its various elements certain characteristics which the central, highly clarified elements could not possess unless all of the secondary factors had been properly coördinated with, or better, subordinated to them.

The point which we fixate we recognize as having position in space, as having relation to the other factors that enter into visual experience, and the whole group of factors is recognized as completely mastered in a single unitary percept. The unity is by no means the same as the inclusiveness. It is a unique and highly developed fact in which the different sensory elements are made to contribute to a single clearly marked phase of conscious experience.

It has frequently been observed that the movement of binocular coördination in infants is later in its development than the sympathetic lateral movements of the eyes. It is probably true in view of these observations that there is a certain period of infancy during which recognition of position in depth is undeveloped. We certainly can not assume that the infant whose two eyes are not yet coördinated in binocular adjustment

is deprived of the mass of retinal sensations that come to him from the ordinary impact of light upon his two organs of sense. There must be unlimited confusion in his experience because of the tendency which many points in the field of vision have to attract lateral movements to themselves, and when these lateral movements are undertaken they not only do not prove satisfactory, because they bring new confusing elements into experience, but they furnish a motive for experiments in new types of ocular adjustments which are distinct from those which arise easily and reflexly. The infant, in other words, must have a vague consciousness that his adjustments are increasingly inefficient, when he moves his eyes in simple lateral movements in the presence of two bright points which lie at different distances from him. To hold that the infant is in any sense of the word clearly conscious of what we have here stated in an abstract logical form is, of course, a fallacy which can easily be guarded against by reformulating our statement and putting it in impersonal terms. We may say that there is confusion in consciousness from the time the infant comes in contact with two bright points at different distances in depth. This increasing confusion must continually excite new motor adjustments until by some chance a form of motor adjustment appears which will bring the two foveas into the familiar relation to the bright points. The analogy suggested in Fig. 115 has already been pointed out as perhaps the best analogy with which to inforce this point. If the infant were not capable of recognizing the confusion in experience in some fashion or other, he would simply go forward with the sympathetic lateral movements which are the easy forms of adjustment. There is no reason to believe that he would ever rise above this level any more than the subject reported in the photographs in Figs. 105 and 106 who, having no retinal images to become confused, has no motive whatsoever for undertaking new forms of visual adjustment above those of the simple sympathetic type.

The confusion in the earliest stages of impression must be recognized as a motive for effort until there shall be substituted for the confused mass of experience a more satisfactory or highly selected type of experience. This organized mass of

experience will be no less inclusive than at first, but it will be more completely unified. Furthermore, it should be noticed that the rearrangement is not a merely static process. It does not consist in the adding together of sensory factors which are alike in quality, but there is a progressive combination of all of the different factors of sensation and of activity until the reflex tendencies aroused by different sensations shall be combined into an all-inclusive unitary tendency. Such a formula as this does not emphasize the sensory qualities of two retinal stimulations, it emphasizes rather the importance and necessity of a single adjustment which shall bring together stimulations through a single motor adjustment. The formula is both sensory and motor. It is a formula dealing with coördination of elements rather than with the elements themselves. The general grounds for the adoption of such a formula as this have been dealt with by the writer in an earlier paper.[1] The formula serves so admirably to explain the results in this series of photographs and at the same time clears up so completely the difficulties which interfere with any effort to give a purely analytical account of binocular fusion, that the present discussion must be accepted not merely as an application but also as a confirmation of the position assumed in the earlier paper.

[1] Vol. No. I., New Series, *Yale Psychological Studies,* pp. 199–266.

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AN EXPERIMENTAL STUDY OF VISUAL FIXATION

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TABLE OF CONTENTS.

CHAPTER I. The Fixation Field.

CHAPTER II. Adequate Fixation.

CHAPTER III. The Complication of the Visual Process During Fixation.

CHAPTER IV. THE ORGANIZATION OF RETINAL ELEMENTS.

APPENDIX. THE TECHNIQUE OF RECORDING THE EYE MOVEMENTS.

CUTS, PLATES AND TABLES.

AN EXPERIMENTAL STUDY OF VISUAL FIXATION.

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Chapter I. The Fixation Field.

§ 1. *Visual Fixation and Its Anomalies.*

Every complete visual act involves both eye movement and fixation. The two factors alternate when eye movements of the 'first type'[1] (*i. e.*, simple rapid eye movement) separate moments of significant stimulation. They are concurrent in compensatory and pursuit movements. And finally, they are complexly related in the relatively slow movements of convergence. Whatever their mutual relationship, however, in normal vision it would be absurd to regard either one or the other as an end in itself. Both eye movement and fixation are means to the attainment of a satisfactory visual impression; and since both are the products of muscular activity, fixation, even if it were absolute, would be only a limiting instance of eye movement, while the study of the latter finds its natural complement in a study of the consequent fixation.

The close relationship between eye movement and fixation is reflected in the uncertainty of traditional terminology. The term 'fixation' is frequently used to cover the entire process of visual adjustment, including the antecedent eye movement. In general, however, fixation appears to mean that the point of regard remains relatively unchanged within the visual field. In this sense the point of regard is commonly called the fixation point. It is obviously in this sense that I have used the term. On the other hand it is a commonplace of physiological optics that the globe of the eye is never fixed in the sense that it is free from minute irregular movements. The latter become

[1] *American Journal of Physiology*, Vol. VIII., 301, fol.

distressingly obvious when one attempts quantitative observations on the living globe. In general, however, these irregular eye movements have been regarded as accidental variations from a normal constant fixation, and the latter has been somewhat uncritically regarded as the type. Recent investigations of the eye movements, however, justify the assertion that absolute fixation of the globe does not exist in normal vision.

The first systematic study of these minute involuntary eye movements with which I am acquainted was made by Delabarre. Unquestionably the most complete account of the irregularities of fixation was published by McAllister in the Yale Studies.[1] Both investigators found that during supposed fixation there is continuous eye movement over a variable area of very appreciable extent.

McAllister found that the amplitude of these involuntary eye movements varies with the character of the object of regard and its surroundings. It also varies with the individual, and is different for the different eyes of the same individual, while no two successive fixations of the same point by the same eye are ever exactly the same. In general, however, his diagrams indicate an area of wandering regard corresponding to somewhat less than one degree of eye movement.

McAllister's results obviously justify Judd's use of the term *fixation*. On the other hand, in spite of the fact of continuous movement, it seems worth while to distinguish between those purposive eye movements that condition clearer vision, and the minute non-purposive oscillations that occur during moments of comparative quiet. The justification for the discrimination is the difference of function. But it should be remembered that whenever we speak either of ocular or of visual '*fixation*' we mean the moments of restricted mobility which have heretofore commonly been called by that name.

A simple and fairly satisfactory class demonstration that the ocular fixation movements (fixation pseudonystagmus) really correspond to unconscious and involuntary movements of the point of regard may be produced by projecting a good negative after-image of an illuminated wedge on a sheet of

[1] Monograph Supplements of the PSYCHOLOGICAL REVIEW, Vol. VII., pp. 17–53.

cross-section paper. Even the utmost effort to 'fixate' some point on the cross-section paper, will not prevent a fatal inability to keep the after-image quiet. The experiment is usually surprising enough to the student, since these rapid fixation movements usually give absolutely no introspective evidence of their existence, although they persist in spite of every effort to hold the eyes rigid.

Unfortunately, two factors coöperate to partially invalidate this after-image method as a means of investigating the normal extent of the fixation movements. First, the obvious wandering of the after-image itself gives immediate warning of the lapsed fixation. The lapse is more quickly corrected, and consequently the amplitude of the fixation movements is much less than that discovered by objective registration. On the other hand, a more or less strongly marked tendency to ignore the nominal fixation mark and to attempt to fixate some part of the after-image leads to occasional breaks in the rhythmic oscillation of the after-image about the fixation mark. Without warning, and without intent, the after-image suddenly darts off in the direction of its center of gravity, and the true fixation is renewed only after an appreciable interval. These two invalidating factors are apparently opposed in direction, but they are not mutually inhibitory, since the second factor is intermittent and always occurs in selected directions.

In my own case the diameter of the area of wandering fixation as measured by the above inaccurate method is about 10′. The amplitude of the occasional movement in the direction of the center of the after-image is more variable but in general it lies within a circle whose diameter is half a degree of eye movement. Both forms of fixation movements, but more especially the latter, seem to be increased by visual fatigue. The former variation, on the other hand, is much more pronounced after bodily exertion. This fact together with a curious regularity in some of my records lead to the conjecture that one factor in the rhythmic fixation movements is due to vasomotor changes. The main theoretical value of the after-image demonstration is the conclusive evidence which it furnishes that the rhythmic fixation movements involve actual

movements of the point of regard and are not merely misinterpreted head movements.

Since in the strict sense of the word there is no absolute fixation it follows that there is no true fixation point. Without disturbing the time honored equivalence of the terms ' fixation point ' and ' point of regard,' I would propose the term *fixation field* to include that part of the field of regard which is traversed by the point of regard in the fixation movements. The new term is analogous to the term field of regard. It immediately sugests fixation movements, and is not otherwise in common use.

§ 2. *The Physical Origin of Fixation Movements.*

The question as to the causes of the fixation movements has never received systematic investigation, as far as I am aware. I am not sure it deserves systematic investigation, but there are some factors at least that are significant. Among the general limitations to strict ocular fixation doubtless the most obvious is the nature of muscular action. Opposed muscle tensions are never absolutely balanced for any length of time in normal life. No part of the body is rigidly motionless, and the movements of the more mobile members need no refined demonstration. The involuntary movements of the head and hand bear a close family resemblance to ocular fixation movements. But in addition to the more or less intermittent waves of excitement which the eye muscles doubtless share with all other contractile tissue, it is obvious that any lack of rigidity of the head and trunk muscles must result in passive displacement of the eyes and a consequent disturbance of visual fixation unless the movements of the head and trunk are accurately compensated by delicately adjusted coördinate eye movements.

Involuntary movements of the head do not lend themselves readily to mechanical registration. Direct tracings must lose many of the more minute movements. And even a system of levers is hardly sufficient to ensure an adequate account of the complex variations in the axis of rotation. A fairly good demonstration of the nature and amplitude of those especial head movements that would most certainly produce passive

eye movements may be made with a pair of smoked glasses. It is a fact, familiar enough to all those who have the misfortune to wear glasses, that the reflections of objects behind the observer may on occasion superpose themselves on objects seen through the glasses. But a movement of the head causes a movement of the reflection quite different from the apparent movement of the object seen directly in front, through the glass. Smoked glass increases the relative brilliancy of the reflection. If one glass in a pair of smoked spectacles is bent slightly away from the eye, the reflection of an artificial window may be made to cover a piece of cross-section paper seen through the glasses. Lateral displacement of the head will be shown by the apparent movement of the artificial window reflection over the cross-section paper. And the amount of the apparent displacement can be read off immediately in divisions of the paper. Somewhat less satisfactorily direct observation of the head movements may be made by attaching a light rod to a Helmholtz mouthpiece. The apparent movement of the end of the rod across the cross-section paper may be reduced to tangents of the angle of head movement. But the weight of the long pointer introduces a foreign element; and the mouth bit arrangement precluded the use of the device for testing the reliability of various head rests. Moreover, the former method has the advantage of giving readings double the amplitude of the actual angular displacement of the head.

Unfortunately both the above methods are subject to much the same criticism that invalidated the after-image test of fixation movements, viz., the data, on which the value of the tests depends, furnish an unusually delicate indication that the head has moved and constitute an unusually accurate motive for corrective movement.

Notwithstanding these unusual motives for the limitation of head movements they are by no means eliminated. On the contrary the practically continuous movements show at least three interacting factors.

(*a*) The most regular movements consist of a succession of incisive, rhythmical jerks, of nearly constant amplitude under

similar conditions. In character and frequency these movements immediately qualified as pulse movements. They show the typically sharp pulse stroke and the slower, more irregular recovery. They appear under the most diverse positions of the head, but the direction of the initial stroke and the consequent recovery change notably under different conditions. When my head is approximately at its primary position the greatest elongation of the main stroke is vertical. When my head is turned 50° or 60° to one side the main stroke is oblique.

(*b*) Beside the rhythmic pulse movements there are less frequent, more irregular disturbances coincident with breathing.

(*c*) Finally, even when the respiration movements are temporarily inhibited, there are irregular drifting movements interacting with the pulse movements.

The amplitude of the combined movements varies with the general excitement, and with the amount and character of support furnished the trunk and head. When sitting quietly on a stool without back rest the rhythmic pulse movements fell within a circle whose total diameter was approximately 10′ of head movement. The occasional extreme movements, coincident with respiration, under the same circumstances, fell within a circle whose diameter was 20′ of head movement.

No support for the trunk either at the back or front modified to any appreciable degree the amplitude of the rhythmic pulse movements (type *a*) ; but merely resting the head on the hands reduced all the movements somewhat. When the head was pressed firmly against a rigid side support, the total movement in five minutes including half a minute of eye closure, was 10′ in a vertical direction, and 5′ in the horizontal. The rhythmic pulse movement was very small, approximately 1′. Using the form of head rest that I ordinarily employ to hold the head for photographing the eye movements, reduces the rhythmic movements of the reflection to a barely perceptible tremor the amplitude of which could not readily be estimated but seemed to reduce to perhaps 15″–30″. Actual photographs of these tremors, Plate I., Fig. 2, reduce to 111/291 of one minute of head movement. Breathing movements on the other hand and the irregular drifts whose source is legion,

were never even approximately eliminated. With care my head rest will keep the head within a total movement of 5′ in five minutes. In actual practice, however, without the additional control of the moving reflection, I find that my photographs indicate a total head movement of somewhat greater amplitude.

In total amount, if not in kind, the head movements would account for all the fixation movements of the point of regard, if the eye were immovable in its orbit. But they would account for more than actually exists. Furthermore, while the amplitude of the head movements steadily decreases with increasing stability of the head rest, the amplitude of the fixation movements of the point of regard is relatively independent of the amplitude of the head movements. Indeed I have repeatedly noted an increase in the oscillations of the point of regard concurrent with the fixing of the head firmly in the head rest. This, however, was an evanescent phenomenon which did not lend itself readily to accurate determination. The important fact remains that when the pulse movements of the head are reduced to a mere quiver the amplitude of the fixation movements of the point of regard remains essentially unchanged. Equally significant, on the other hand, is the evidence that movements of the head and eyes do not directly correspond. See Plate I., Fig. 2.

Besides the fixation movements resulting from pulse and respiration, the eyes suffer passive displacement with every movement of the head and body. For a great variety of such somatic movements there exist coördinate compensatory eye movements through which, without reaction interval, a surprisingly accurate fixation is maintained. So regular and persistent are these coördinate compensatory eye movements[1] that, except when the eyes are strongly converged, it is impossible for me to move my head with the eyes fixed in their sockets, *i. e.*, so that the eyes move directly with the head. That the normal coördinate compensatory eye movements are really coördinate with the head movements and are not reactions may be shown by a simple experiment entirely without the aid of registering

[1] *Am. Jr. of Physiology*, Vol. VIII., p. 322, fol.

apparatus. If three or four lines of print are placed at a comfortable reading distance before a subject, it will be found that head movements of moderate amplitude and velocity do not seriously interfere with the process of reading, *i. e.*, notwithstanding the head movements the fixation pauses of reading are sufficiently well maintained to prevent blurring of the printed matter. If, on the other hand, the same lines of print are attached to a light rod and the latter be held firmly in the mouth so that the whole moves with the head, it will be found that the process of reading becomes difficult and even impossible as the head moves back and forth. Now it is obvious that if the compensatory eye movements were reactions to the apparent movement of the field of view in the first case then there would be some considerable blurring of the print before the eye movement got under way, which is contrary to fact. Conversely, the absence of all apparent movement of the object of regard, with respect to the head, would produce no stimulus for reactive eye movements, but the coördinate compensatory eye movements actually occur just the same even though they seriously disturb or even entirely inhibit the process of reading, by blurring the field of vision.

The same results are obtained if a row of black dots is used instead of letters. Indeed the latter arrangement has some advantages over the letters since the character of the visual disturbance is even more apparent. The experiment conclusively shows that during normal head movements the eyes do not remain fixed in their sockets. It shows furthermore that the eye movements which compensate for normal head movements are not reactions to the apparent movement of the field of view, but are in fact, as we maintained, coördinate to the head movement, and have their origin in the same impulse that moves the head. In one way at least these coördinate compensatory eye movements are of unique theoretical interest, since in them more completely than in any other motor experience with which I am acquainted, the eye movements and the movements of the head and body are united into a thoroughly organized motor system. When one realizes that coördinate compensatory eye movements occur with every step

in walking, with every movement of the legs and arms, as well as with every movement of the head, it is obvious that we have come upon a coördinating mechanism that is thoroughly capable of explaining the intimate correspondence between tactual and visual space. These facts will serve us later when we discuss the spatial organization of the retina in Chapter IV.

There are many bodily movements, on the other hand, that have no coördinate compensatory eye movements. Passive movements of the head obviously could have no true coördinate compensatory eye movements, and as a matter of fact they do not have them. If, when the subject is seated in a revolving chair while the reading matter is attached to an independent support, the chair is rapidly rocked back and forth, producing rapid passive movements of the body and head, reading will be grossly disturbed. The experiment is best if irregular oscillations are made cr. 4–5 per sec. If on the other hand the printed matter is attached to the chair so as to move with the head in its passive movements there is little or no disturbance of the reading process. The results of the experiments with the passive movements of the head are thus seen to be directly the reverse of those with voluntary movements. The spasmodic eye movements that tend to appear during passive movement of the head even when the eyes are closed, show a true reaction interval and may be regarded as reactions to some non-visual perception of movement. Furthermore, voluntary movements of the trunk from the hips, in which movement the head is held rigidly in line with the trunk, show no coördinate compensatory eye movements. Finally, I can detect no true coördinate compensatory eye movements for the indefinite, non-rhythmic, and irregular head movements.

In view of all these facts I venture the generalization that, notwithstanding the delicate coördination between some head and body movements and the eye movements, there still exist in the adult, probably in the pulse movements and surely in the irregular head and body movements, entirely normal and continuous physical disturbances of visual fixation. I have purposely left out of consideration all abnormal disturbances such as occur in dizziness, nystagmus, etc.

§ 3. *Visual Motives for Fixation Movements.*

Besides the physical sources of fixation movements there seems to be some evidence that there are visual motives, though I confess that for one of these the evidence does not seem altogether conclusive. Our problem must not be confused with the question whether or not there are visual motives to follow a moving object. Of that there can be no doubt. Neither should it be confused with the question as to the natural limitations of the visual control of fixation. That will call for special discussion later. Our present problem concerns the fixation of a motionless object; and the question is whether there are or may be visual motives to make such fixation relatively unstable. I believe such a motive exists in retinal fatigue, and the involuntary shifting of the functional center of the retina. It is now a well-known fact that a single point on a plain field is the worst possible fixation mark, and permits the widest possible fixation movements. That it actually induces fixation movements would be difficult to demonstrate. But I believe it may and probably does on the following grounds: There is little doubt that one of the most important signs of satisfactory fixation is the clearness and sharpness of a visual impression. Now it is evident that if by some perfect adjustment a point of light exactly 1′ in diameter should fall continuously on a single retinal element at the fovea, the development of fatigue in that retinal element would bring about a gradual fading of the impression until it disappeared. At every moment of our hypothetical case, after fatigue had set in, it appears that the impression would be clearer and brighter if the stimulus should pass from the original fatigued element to some neighboring non-fatigued retinal area. But the lessened sensitiveness of fatigued retinal elements is the same whether the object is a visual point or a complex of several points. Under ordinary circumstances then clearest vision would be subserved not by such an absolute fixation as should keep the same stimulus constantly on the same retinal elements, but by a more or less rapid substitution of fresh for fatigued retinal elements. A similar function is subserved by winking. Moreover, the natural observation of an object involves more

or less rapid alternation of fixation pauses separated by eye movements, as we look successively at its various parts. If for any reason it became necessary to continue the fixation of a single point indefinitely it certainly does not seem improbable that visual fatigue would operate to produce a shifting of the image from fatigued to less fatigued portions of the fovea. Certainly some of the movements of continuous fixation records are more easily explained by that hypothesis than by any other. See Plate I., Fig. 2. And there is abundant evidence for a shift in the functional center in dark adaptation. Even if we hesitate to admit visual fatigue as a positive motive to produce fixation movements, it must very obviously serve negatively as a limitation of the visual motives which tend to check such fixation movements.

A second visual motive for fixation movements is clearly demonstrable in binocular vision in the correction of inadequate binocular coördination. Movements of convergence and divergence complicate even the simplest changes in the position of the line of regard. In some cases, as Judd has shown in the only accurate discussion of convergence movements ever published,[1] these movements of convergence are coördinate with the rapid movements in connection with which they appear, and occur without adequate reaction time. In other cases as in some photographs which it is not necessary to discuss further in this place, the slow corrective convergence movements may occupy the entire period of nominal fixation, and, at the end at least, they are doubtless reactions to unsatisfactory visual conditions.

It is with some hesitation that I speak of movements of convergence as fixation movements at all. It may be objected that they constitute rather a part of the precondition of fixation than fixation itself. But there is no doubt that they may occur during moments of relatively clear vision, and they thereby differentiate themselves from those eye movements in which there is no new adequate stimulation of the retina, whose only function is to secure more favorable conditions for viewing the new object of interest.

[1] 'Records of Convergence and Divergence,' The PSYCHOLOGICAL REVIEW Monograph Supplements, Vol. VIII., pp. 370, fol.

§ 4. *The Inhibition of Fixation Movements.*

There is no natural guarantee for fixation of any sort. There is no pre-established harmony between any object in the external world and any special part of the human retina. On the contrary every new act of fixation is in consequence of some discreet stimulus to eye movement; and in view of the continuous displacements of the eyes by head and trunk movements, every act of fixation is maintained only by a more or less continuous process of readjustment. There are only two apparent means for stimulating the eye muscles to preserve fixation or to reëstablish a lapsed fixation. One of these means is the physiological coördination which we have seen actually operates only within certain well-defined limits. The other is the direct peripheral stimulus involved in the lapsed fixation itself. The former presupposes prevision of the direction and extent of the head movement and would be helpless to correct either a chance inaccuracy of coördination, or an irregular displacement for which there were no corresponding coördinate compensatory eye movements. In all such cases the reëstablishment of the fixation must depend on some sensory stimulus involved in the lapsed fixation itself. Kinæsthetic data would be inadequate for slight lapses even if they existed. But there is no evidence that kinæsthetic data ever provide the stimulus for reëstablishing even the grossest lapses of fixation. And, in view of the presence of purely visual data corresponding exactly with the only possible motive for ever reëstablishing any fixation, there seems to be little ground, either biological or psychological, for the development and refinement of kinæsthetic control. The visual control shown in our after-image experiment was an artificial affair. As we have already noted, it was complicated by the presence of the well-defined after-image. This is seldom duplicated in natural vision, but it is closely approximated during the fixation of sharp boundaries between contrasting surfaces, when a fringe after-image, often confused with irradiation, may function as a delicate datum for lapsed fixation. It is directly congruent with this general principle that sharp lines tend to restrict fixation movements across them, and conversely, that

the elimination of sharp boundaries as in Myer's contrast experiment increases the tendency to fixation movements. Ordinarily the visual control of fixation lies between these extremes and, as photographic records show, ordinary visual control permits considerably wider fixation movements than our after-image experiment discovered.

Even under the most favorable conditions of visual control the fixation movements are altogether disproportionate to the sensitiveness of the retina to local difference or to the movement of objects across the field of view. At least one element which tends to produce this disproportion is the relatively long ocular reaction time. Obviously no visual clue to a lapsed fixation could act except as a stimulus to an ocular reaction. But since the physical displacement of the line of regard operates not only up to the point where it constitutes a stimulus for corrective eye movement, but is also operative more or less irregularly during the interval between the stimulus and the reactive eye movement; it must follow that, even if the reactive motor impulse were exactly sufficient to correct the lapse at the moment of stimulation, it would effect an exact correction only occasionally and accidentally. Unless the intervening eye movements were sufficiently regular to permit some prevision of their probable extent and direction, there could be no absolute correction unless they accidentally happened to exactly neutralize each other. Apparently some of the movements admit prevision. This appears to be the case with respect to the pulse movements. After the first few pulsations under any given circumstances the head pulses seem to be automatically corrected by short-lived ocular habits. Plate I., Fig. 2. The insufficiency of the correction of the irregular head movements would be directly proportionate, other factors remaining equal, to the length of the ocular reaction time.

§ 5. *The Reaction Time of the Eye.*

The available data relative to the reaction time of the eye to a visual stimulus is very limited. Following a method devised by Professor Erdmann and myself in 1899, I published a series of observations in 1903 giving my own average reaction

time to a peripheral retinal stimulus as 165 σ. Huey confirmed the generalization that the reaction time of the eye is relatively long. The development of photographic registration seemed to give opportunity for relatively exact objective measurements. Two series of photographic records confirmed in general the long ocular reaction time, but both series contained an apparently ineradicable source of error, rendering the results comparatively valueless. The difficulty in these records was the fact that each record consisted of three separate lines on the photographic plate, viz., a time line and a signal line besides the corneal reflection line. Since these lines necessarily differed more or less in their density, there was an obvious source of error in reading the plate, which error was only slightly less than the total width of the slit through which the recording beams fell on the photographic plate. It soon became evident that the number of lines must be reduced and I had already arranged a tuning-fork interruption for the beam of sunlight when Holt reported satisfactory results with the alternating arc light. That solved my difficulty and made it possible to record in a single line, the time, the beginning of the stimulation, and the reaction.

The general arrangement of the apparatus is given in the line drawing Fig. 1. *C.* is an enlarging camera approximately

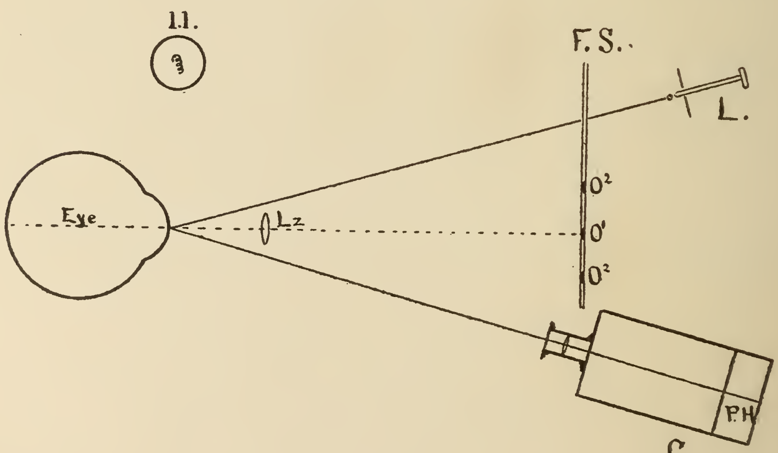

Fig. 1.

4 ft. long. *P.H.* is the box holder containing the mechanism for the regular movement of the plate. *L.* is the alternating current arc light, enclosed in an asbestos-lined box, with a small aperture to confine the light to the reacting eye; this aperture is covered with from one to three thicknesses of pot blue glass to stop out all but the actinic rays. *F.S.* is a falling screen exposure apparatus in which the falling screen is so placed with reference to the arc light and the object that at the same moment that the object is exposed the recording beam of light is flashed to the cornea. O^1 is the preëxposure fixation mark. O^2 is the object stimulus to reaction.

A photographic reproduction of the apparatus in position is given in Plate III., opposite page 93.

The apparatus functions as follows: The observer sits comfortably in front of the camera and fixates an arbitrary mark O^1 on the black shutter of the falling screen exposure apparatus. The whole is brightly illuminated by incandescent electric light *l.l.* Full correction for any visual error is made by placing the proper correcting lens in a standard at *Lz.* The head is held fairly rigid (page 94) by the heavy head frame. After an appropriate signal, when the sensitive plate was allowed to fall slowly, the black shutter holding the preëxposure fixation mark was released and withdrawn by a spring. By a movement of less than half an inch the falling screen simultaneously exposes the object word at O^2 and opens a passage for the recording beam of light. The beginning of the exposure, therefore, is simultaneous with the beginning of the record on the plate. Since the coincidence of the beginning of both the exposure and the record is mechanically conditioned, while the beam of light has neither latent period nor inertia the conditions for recording the beginning of the stimulus are almost ideal. The ocular reaction to the peripheral stimulation is recorded by the beginning of obliquity in the photographic line. Here again the registration is accomplished without latent time or any other constant error. The form of exposure of the word stimulus is not altogether satisfactory, involving as it does, the uncovering of the word by a falling screen. The fault was difficult to obviate. It was minimized

PLATE I.

Text to Plate I.

FIG. 1. Line drawing from the photographic record of the horizontal displacement of the corneal reflection during the supposed fixation of a dimly lighted black dot on a white field. Both head movements and eye movements are included in the record. The pulse movements are clearly visible, and are indicated by dots.

FIG. 2. Line drawing of an enlarged record of horizontal head and eye movements during the supposed fixation of a well-lighted black dot on a white field. The head line at the left shows the rhythmic pulse movements and the slower drifting movements. The eye line at the right shows how in prolonged fixation the pulse movements of the head may be completely compensated.

FIG. 3 is an enlarged record of a photographically registered reaction to a peripherally exposed word. The beginning of the record, reading up, is the beginning of the stimulus. The first break in the line is an artificial break to indicate which way the plate is to be read. The beginning of the eye reaction to fixate the exposed word is shown by the oblique displacement of the record, *C*. The second smaller displacement, *D*, is a corrective movement, showing that the first movement of the eye failed to carry it far enough. The beginning of the speech reaction is shown by the ending of the record, *E*.

FIG. 4 is an enlarged record of the ocular reaction to a moving pendulum. The beginning of the record is coincident with the beginning of the movement of the pendulum. The first lateral displacement in the record shows the first indication of reactive pursuit movement. Successive lateral displacements show the typical corrective movements found at the beginning of all pendulum pursuit movements. It will be noticed that an approximation of the pursuit begins immediately after the first rapid reaction movement.

FIG. 5 is an enlarged record of pendular pursuit movements, after the initial irregularities have been overcome and the eye has settled down to the true pursuit. It will be noticed that even here there are slight corrective movements, occurring chiefly at the center of the arc of oscillation.

FIG. 6 is an enlarged record of the attempted pursuit of the pendulum, when for about one quarter of the arc of swing the pendulum passed behind an opaque screen. The gross disturbance of the pursuit could not be obviated by any amount of effort.

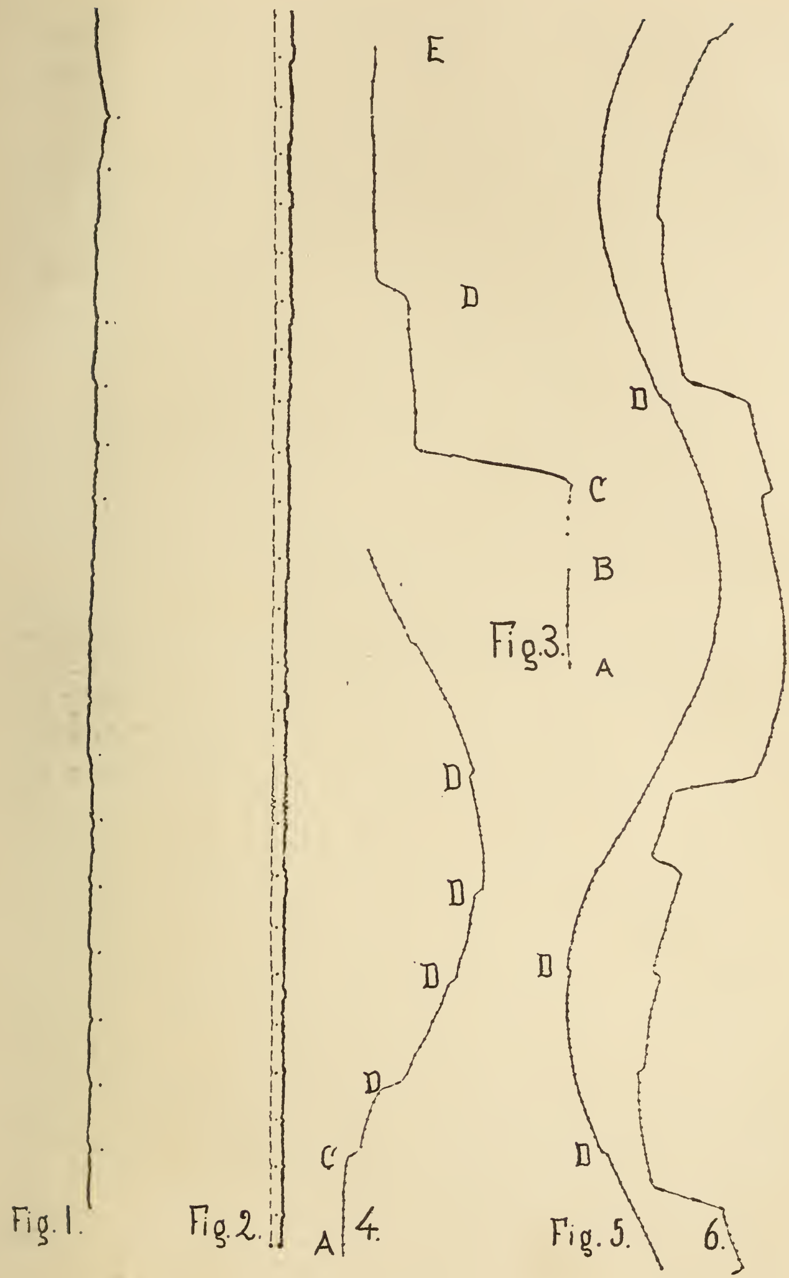

Fig. 1. Fig. 2. Fig. 3. 4. Fig. 5. 6.

PLATE I.

by making the screen operate as rapidly as possible, so that the difference between the exposure of the upper and lower parts of the word was approximately 1 σ. Moreover, the investigation was not primarily a study of the process of reading, so that the form of exposure was of less significance.

The end of the record in the enlarged record reproduced on Plate I., Fig. 3, indicates the conclusion of a speech reaction. The first puff of breath in speaking the exposed word broke an electric circuit by means of a modified Cattell sound key combined with our mouth bit. This break in the circuit released an opaque screen which interrupted the beam of recording light and therewith ended the record.

In figures 3 to 6 the punctiform character of the record line should be observable. These points represent the alternations of the arc light and give the time. A continuous line connects the dots due to the fact that the globule of molten carbon remains at white heat from one alternation to another.

The photochronograph above described has some obvious advantages over all other forms of recording device for reaction experiments in general. Except for the constant drone of the arc light its operation is altogether noiseless, and there is no intrinsic reason why even the drone of the arc light might not be eliminated by placing the lamp in another room and passing the light through a transparent door. The records are indelible and easily read by projection. Since one plate 5 x 7 in. will serve from 10 to 20 reactions, it is not prohibitively expensive. The main limitation to its general use is the variation in the alternating current. This is a real difficulty when great accuracy is demanded, but it may be entirely obviated by using a direct-current lamp and interrupting the beam by a tuning fork of known frequency. I have tested this arrangement in a series of measurements and find it fairly satisfactory. In the tests I made of our alternating current it proved reliable to less than half an alternation a second. Its mean value was 61.6 complete phases per second.

One further factor in these reaction experiments deserves consideration. It is a familiar fact that an intense peripheral light constitutes a more or less serious disturbance of all the

retinal processes. Unfortunately, however, a highly actinic beam of light is indispensable for photographic registration, and the problem reduced itself to producing a highly actinic beam of light with the least physiological effect. This was accomplished by stopping down the arc light by plates of pot blue glass. This blue glass only slightly reduces the effect of the light on the plate while it reduces the visible effect of the arc light from the annoying glare of the naked arc, which dimmed the whole field of view, to a comparatively insignificant appearing glow, which scarcely had sufficient physiological intensity to provide a perceptible after-image for experiments described in the Appendix. In respect to its physiological effect as well as in general convenience I found the arc light more satisfactory than sunlight.

The result of the simple eye reactions to words of four letters is given below in Table I. No records are omitted.

Table I.

A. When the center of the 41′ word stimulus was 45′ or 1° 30′ either to the left or to the right of the line of primary fixation.

No.	Average.	Lowest.	Highest.	Mean Variation.
14	166.6 σ	138 σ	186.6 σ	13.6 σ

B. When word stimulus was 45′ or 1° 30′ to the right only.

No.	Average.	Lowest.	Highest.	Mean Variation.
17	151 σ	130 σ	163 σ	9.9 σ

C. When word stimulus was 4° 30′ or 6° to the right only.

No.	Average.	Lowest.	Highest.	Mean Variation.
9	181 σ	146 σ	252 σ	19.1 σ

Notwithstanding the general reliability of these results, it would obviously be inadmissible to apply them directly to the correction of a lapsed fixation. They are, to be sure, eye reactions, but they are reactions to a complex new peripheral stimulation, and involve a change of attention from the preëxposure object to the exposed word. It is certainly not *a priori* impossible that reactions to a lapsed fixation would be shorter. In view of these considerations I arranged a control experiment which approximated rather closely the conditions of a lapsed fixation. While the subject fixated a sharp point of light on the bob of a second pendulum the latter was set in motion by

breaking an electric circuit. The same circuit also held a shutter over the beam of light, so that coincidentally with the beginning of the movement of the pendulum the record began. Plate I., Fig. 4, reproduces a typical record of these control experiments. Unfortunately they are too few to constitute a proper demonstration, but it is significant that the one reproduced shows a reaction time of over 180 σ. Records of the pendulum reactions for other eyes than my own are more numerous but I have no eye reactions to the moving pendulum less than the minimum reaction to an exposed word, *i. e.*, 130 σ.

In all eye reactions there is a certain amount of pre-reaction eye movements. In view of the prevalence of fixation movements, even under the most favorable circumstances, these pre-reaction movements are inevitable. They constitute a beautiful example of the unstable equilibrium discussed by Judd in the first volume of the Yale Studies, and in this case at least some of the factors producing the disturbance are known. But the pre-reaction eye movements constitute a serious menace to the accurate reading of the reaction records. In the present instance, moreover, there is apt to be an additional complication in the increased tendency to head movement that accompanies reaction to the pendulum movements. This is particularly noticeable in untrained subjects. The apparent paradox seems to obtain that the head reaction is considerably more rapid than the eye reaction.

As far as the visual factors are concerned, however, the pendulum control reaction is satisfactory enough. The effort was consistent throughout to maintain the fixation of a well-defined point. The lapsing of that fixation, in this case by a movement of the pendulum, involved no wandering of the attention. The reacting eye movement was directly demanded by the original effort to maintain fixation. In this case, as in simple fixation, it occurred in response to the necessity to reëstablish a lapsed fixation.

A study of the records of fixation movements shows some rapid movements occurring in succession without the intervention of full reaction intervals. At first sight this might appear to indicate some form of ocular reaction more rapid

than that which we have succeeded in obtaining experimentally. Naturally this is not absolutely impossible. But, on the other hand, there is abundant evidence, which we shall regard in detail in the chapter on the complication of the visual process, that successive stimuli may initiate reactive movements of the eyes, and that the latter may appear successively in approximately the same tact as the stimuli. Moreover the reactive correction of the slow drifts is not more frequent than the reaction experiments would lead one to expect.

§ 6. *The Fixation Object.*

In view of the persistent irregularities of normal fixation, in view of the numerous physiological and psychological motives for fixation movements, and finally, in view of the slowness and general inadequacy of the correction of lapsed fixations, the traditional assumption of an identical anatomical and functional center of the retina, to which all visual processes are referred or referable, is untenable. A functional center in the sense of a retinal *point* which normally corresponds with a *point* of interest does not exist. Even if for the sake of the discussion we could admit that primitive vision involved the sharp differentiation between a central element and all other parts of the retina, there are obvious psychophysical motives which would tend to break down the distinction in the interests of clear vision. The continuous rhythmic movements of pulse and respiration would occasion continuous illusions of motion, which the increased amplitude of movement during the long ocular reaction intervals would further exaggerate. Furthermore, the perpetual inaccuracy of what subjectively passes for fixation and refixation, must tend to break down the too accurate differentiation. But there is no evidence for such a con-natal preadjustment of vision. Indeed all the evidence is directly opposed to it. And there is certainly no adequate motive for its development.

On the contrary, and in addition to the grounds already exploited, there is experimental evidence that for any given object of regard several retinal positions are practically equivalent.

TABLE II.

occasional	gratifying
thoughts	statements
judicious	liberty
practicable	popular
experts	accuracy
the articles	experts
paragraphs	extent
degree	extended
to the notes	practicable
series	arti-
h	conscience
e	thoughts
needful	paragraphs
week after week	judicious
pages that follow	x

The experimental evidence on which I venture this assertion is a series of experiments to determine the natural fixation points of words and groups of words. In recognition of the extreme difficulty of referring any definite point of an objective record, by whatever method it is obtained, to any definite point in the field of regard, I used the only subjective method which is absolutely reliable and trustworthy for the determination of isolated fixation points, viz., the after-image method. A good after-image was obtained by the continuous fixation of

the apex of a brightly illuminated wedge. Then sitting at a convenient reading distance from an upright standard the subject uncovered a word held at about fifteen degrees to one side of the wedge. The word configuration was clearly seen while the word was still illegible. Direct fixation of the word for a moment gave the word percept while directly under or about some part of the word a clear after-image of the wedge indicated the point at which the point of regard had actually rested.

The result of the fixation experiment is given in detail in Table II. The same words and word groups were exposed in three different series. A line above the word indicates the first series. A dotted line indicates the second. A line below the word indicates the third. Any combination of these marks indicates coincidence of the fixation in two series. A large number of control experiments by myself and students gave similar results.

These results are in direct agreement with the wide variations in the successive fixations of the same object that have been noted by Stratton, Dearborn, McAllister, and Judd. These variations may not be regarded as accidental errors symmetrically grouped around a theoretically perfect fixation. They must be regarded as a chance distribution over relatively indifferent retinal elements. Each case is a perfect fixation, not in the sense that some objective point falls in each case on the anatomical center of the retina, but in the sense that the object of interest is brought to a retinal area of clear vision. Anything more would be useless exactitude and nature is seldom uselessly exact. The psychophysiological dogma that there is a tendency to transfer every peripheral stimulus to a fixed punctiform retinal center is a myth. There is no punctiform functional center of the retina. The functional center varies in individual cases. It may be a larger or a smaller area according to the character of the object of regard and the corresponding extent of the area of clear vision. Only artificially is the peripheral object of regard a punctiform object. Usually it occupies an area of appreciable extent, and the object of fixation isn't a point but an area. The psychology of reading has had a long fight against the preconception that

the object of regard must fall on the fovea, and I think there is still a tendency to give undue prominence to the location of the center of any object of regard. The location of the 'point of regard' within an object of regard is valuable, it seems to me, rather because it gives some indication of the nature and extent of the object of regard than because it locates the position of a schematic point of fixation.

As a matter of fact, fixation is confined to no particular part of the retina. The possibility of extra-foveal fixation is abundantly proven for specific circumstances. Aside from the unusual cases of the development of artificial fixation areas, such as the case reported by Storch,[1] normal extra-foveal fixation is a familiar fact of dark adaptation. Moreover the greater part of any fixation object is extra-foveal. To be more specific: if this page is held 12″ from the eye, the fovea will be covered by the enclosed part of an *a* and the entire macula will be covered by three or four letters. Now no one would venture to assert that the significant part of the stimulation in any reading pause was given in an area smaller than the enclosed part of an *a*. Practically all the significant stimulation of the retina in any reading pause then is extra-foveal. To insist that a fraction of a letter is fixated at any reading pause would mean only that an imaginary line called the line of regard passes through that letter, or rather it means that at any given instant the line of regard passes through some point of that letter. That some particular point of the letter determined the extent of the prefixation movement would be as absurd as to insist that since the line of regard passed through a point of the letter consequently that point constituted the real object of regard and the immediate object of perception.

Not only does the greater part of any natural object of regard lie outside the fovea, but there is direct experimental evidence that extra-foveal vision may be the determining factor in maintaining fixation.

If a narrow strip of cardboard be interposed between one eye and the object of regard situated some feet distant on the opposite wall of a room, it may be made to cover several de-

[1] *Zeit f. Psy. and Phys. d. Sinnesorgane,* XXVI., 201–226.

grees of the field of view without preventing binocular adjustments sufficient to produce a fusion of the congruent parts of the two fields of view. This is the more remarkable in my own case since I have a slight hyperexophoria. Whatever it is that determines the position of the eye before which the interference is placed, it must be something aside from both muscular balance and foveal stimulation, and that something must originate in the influence of extra-foveal corresponding points.

A still more frappant demonstration of the extra-foveal determination of the position of the eye is furnished by a chance observation. While I was studying the rhythmic disturbances in fixation by means of the after-image method, I tried a number of backgrounds to see if they might influence the accuracy of fixation. Among other expedients, I projected the after-image on a background of cross-section paper seen through an aperture in a similar screen which was more heavily marked and suspended on a pendulum. On setting the pendulum lightly in motion I noticed wider oscillations of the after-image than before. These wider oscillations persistently followed the tact of the pendulum and covered substantially the same amplitude. Notwithstanding persistent effort to fixate a point of the background that was never covered by the swinging screen, I found it absolutely impossible to avoid following in some measure the movements of the pendulum screen. For the best results the pendulum screen should be heavily marked by some prominent figures and the aperture should not be over ten degrees. I used a second pendulum. It seemed and still seems a striking proof of the influence of extra-foveal and extra-macula determinants of fixation. Doubtless the same principle underlies the waterfall illusion.

Somewhat more exact data are obtained by the records of pendular pursuit movements. If the eye attempts to follow an object of regard which is attached to a moving pendulum, after a relatively short preparation period extending approximately through one complete oscillation of the pendulum, the eye catches the tact of the pendulum and the pursuit movement is broken by relatively few rapid corrective movements. A record of the pendular pursuit movement in its first stage is

given on page 17, Plate I., Fig. 4. The advanced stage is given in the fifth figure of the same plate. If now a screen be placed between the eye and the pendulum so as to shut out from view the middle of each oscillation, no amount of effort can force the eyes to maintain the tact of the pendulum. Plate I., Fig. 6. If on the other hand the screen is so placed as to hide only a part of the bob including the fixation mark, the interruptions again become insignificant.

In view of all the evidence I venture the following generalizations:

The hypothesis of an intra-foveal fixation point, differentiated from all other retinal points as the point of clearest vision, to which all other retinal points are related by a system of motor impulses, which are calculated to bring every point of interest to the fixation point, may perhaps be schematically helpful but it does not represent the facts.

Every field of vision on the other hand embraces a clearer and more distinct central area whose extent varies with the character of the object of regard. But the area of clear vision merges into a less clear and distinct peripheral area without sharp boundaries.

It seems to me that the psychological value of determining the positions of the line of regard in the succession of fixations which constitute the moments of significant stimulation in normal vision, is not to determine the position of the scientific abstraction called the point of regard but to determine the center of the more or less extensive area of clear vision.

It seems to me, farther, that the real problems of psychological optics are not connected with the mythical fixation point so much as with the interrelation between the areas of more and less distinct vision.

Chapter II. Adequate Fixation.

§ 1. *The Clearing-up Process.*

A fixation may be called adequate when, under the particular circumstances in which it is operative, it is sufficiently accurate and sufficiently long to condition a clear perception of the object of regard. The adequacy of a fixation is a relative matter, depending not only on the character of the fixation process but also in part on central conditions, and in part on a number of peripheral circumstances, such as the amount and quality of the illumination of the object and its background, the visual angle which the object subtends, and the nature and illumination of the pre- and post-exposure fields. A complete analysis of the relative influence of these various factors would be a most desirable piece of experimental work for which considerable material is already at hand. Our own problem is less pretentious. In all these possible complications there is a relatively simple and practical problem of how rapidly under otherwise favorable conditions of normal vision adequate fixation pauses may succeed one another. Notwithstanding the relative simplicity of the problem, and its obvious practical bearings on the normal limitation of the succession of discreet visual acts, our material information with regard to it is scanty.

In every succession of fixation pauses and eye movements each new fixation involves a relatively thoroughgoing rearrangement of the retinal stimulation. Irrespective of possible objective changes within the visual field each new fixation is little short of a retinal revolution. Yet, however complete the change, the new retinal processes develop on the ruins of its antecedents. The previous stimulation passes grudgingly and unevenly, often leaving particularly stubborn residua to contest control. Conversely, each new stimulation has a latent period of incubation, and a period of more or less incompletely inhibited development, before it reaches full maturity. The history of this development, depending on the inherent strength

of the new stimulation and the strength of the residua it must supplant, constitutes a kind of monocular *Wettstreit*. On the one hand it appears as a time-consuming interference with the progress of vision. On the other it seems to be one of the most important factors in the perception of motion, and, as I believe, one of the most important sensory conditions in the spatial organization of retinal elements.

From the standpoint of the persistent past the process has been discussed under the general head of after-images, or the after-effects of stimulation. But the duration of the after-effect depends not only on the duration and intensity of the antecedent stimulation, but also on the character of the interrupting stimulus.

From the standpoint of the new stimulus, different phases of the process have found more or less adequate discussion, such as latent time, retinal inertia, the development of retinal stimulation, and the clearing-up process. The latter name is expressive and may serve very well as an inclusive term for the whole process, which, as far as I am aware, has never received systematic investigation.

While after-image and clearing-up process seem to represent the same event from opposite ends, yet there is good reason for a differentiated discussion. A well-defined after-image may persist throughout a number of successive fixations, entirely inhibiting the clearing up of some particular part of the successive fields; or again, the after-image may show a sort of intermittent existence, passing entirely unnoticed during some fixations and dominating again over its particular part of the visual field during others. On the other hand typical clearing-up periods usually pass entirely unnoticed. They are generally ephemeral disturbances of clear vision, rarely submitting to direct observation.

Physiologically speaking, the development of the new excitation is as important as the disappearance of the old. Indeed it is only by reference to a more or less inhibited new physiological stimulus that we can speak of an after-image at all. Psychologically the clearing up of the new impression is more significant since the new impression is naturally the

object of interest. Finally in the last analysis the clearing-up interval must be something more than the mere obverse of a disappearing after-image, since after-images themselves often show analogous periods of incubation and development, emerging from a hazy confusion into full brilliancy and inhibiting the development of antagonistic impressions.

As far as I am aware the clearing-up interval in normal vision was never formally recognized until theoretical considerations led to its discovery. But while it is seldom a conspicuous accompaniment of the succession of visual stimuli, it is nevertheless a real one and after it has once been discovered it may be observed with every change in the object of regard. It must, however, first be analyzed out of the complex visual process, much as an overtone, which one is trying to hear, is heard best after it has once been analyzed out of the simultaneous acoustic process to which it belongs.

A fairly good class-room demonstration of the clearing-up process may be made by beginning with an exaggerated case. If the subject will look back and forth between a well-lighted window and a wall covered with figured paper, but in moderate shadow, at each new fixation of the wall paper the true values of the different parts of the pattern will be seen to gradually emerge from a confused haze in which under some circumstances recognizable residua of the after-image of the window may be noted. The gradual emergence of the darker field is an exaggerated clearing-up process. Some practice with gradually decreasing differences of illumination of the two fields will enable the student to observe similar if relatively less conspicuous phenomena at every new fixation within any normally complex field of regard.

The fact that the clearing-up process is ordinarily unnoticed serves as a further illustration of the general principle that regularly recurring disturbances of the perceptual processes are habitually disregarded. Another difficulty in the unassisted recognition of the c.-u. p. lies in the fact that it would scarcely come to consciousness at all except in distinction either from a period of confusion or from a clearer view of the same object of regard. A consciousness of confusion however would

hardly be expected from the momentary balance of the opposing stimuli unless attention was especially directed to the phenomenon, or the period was considerably prolonged, both of which conditions are experimentally included in our demonstration. Similarly a lesser degree of clearness would ordinarily pass unnoticed as the attention is regularly claimed by the objective fact rather than by its subjective variations.

Perceived or unperceived the process involves a measurable time interval which is of theoretical as well as practical interest, since, other things being equal, it is the measure of the rapidity with which adequate stimuli may succeed one another.

§ 2. *Minimum Duration of the Clearing-up Process.*

The question of the duration of the clearing-up process is particularly refractory to *exact* experimental solution, since in the nature of things it is indefinite, and variable. There are no sharp boundaries at which the old stimulus may be said to have begun to fade, or the new to have become clear. In a more or less arbitrary way the psychophysical clearing up process may be said to begin with the beginning of the new stimulus. But in view of the purely physiological inertia of the retina this is only relatively exact; while if an inadequate fixation interval occurs between the end of the passing stimulus and the beginning of the new, such for instance as may be said to occur during rapid eye movement, then the clearing up interval, in one of its factors at least, is prolonged backward to the end of the last adequate stimulation. Furthermore, the only sense in which we could speak of an absolutely cleared up impression is that in which we would mean that the immediate psychophysical effect of a stimulation had reached its maximum. But this might occur without a real clearing up of the impression at all, as under the influence of a dominant after-image. An impression is *practically* cleared up on the other hand whenever it is differentiated from its predecessor.

The oldest and one of the least satisfactory tests for the minimal physiological clearing up time is the fusion-flicker of revolving parti-colored disks. This is traditionally regarded as a measure of the duraton of maximum positive after-images.

But in neither case is it adequate. First, because the continuous unconscious and unregulated eye movements make a farce of exact measurements of the angle velocity of the sectors.[1] But principally because the test neither gives time for a free development of the after-image independent of the beginning of new stimulation, nor is there a real clearing up of the several sectors. The colors one sees in threshold flicker are quite different from the real colors of the disk. It has always been a paradox of the traditional view of flicker that, whereas the after-effect of a stimulus increases directly with the illumination, an increase of the illumination of a revolving parti-colored disk increases the flicker. What is actually measured in flicker is the time in which the clearing-up process and the after-effect just fail to balance; and the paradox is a demonstration of the fact that after-image and clearing-up process follow different quantitative laws.

The work of Exner and Baxt on the time necessary for a visual perception is more closely relevant to our problem. Exner's experiments determined the duration of the physical stimulus sufficient to produce a just perceptible clearing up of different objects on a dark pre- and post-exposure field. He found that this depended on four factors; the illumination, the size of the object, the undisturbed duration of the after-image, and the position on the retina. With respect to the first variable, the illumination, it appears that, granted sufficient illumination, there is no lower limit to the exposure interval. Exner reduced his exposures on occasion to 1 σ. Cattell operated with .25 σ as did Erdmann and Dodge. While the electric spark used by Aubert and others is doubtless a minute fraction of these incomprehensibly minute intervals.

Unfortunately none of these results are directly transferable to the natural course of vision, since, except in comparatively rare experiences, such as the lightning flashes during a thunder storm, normal fixation pauses are never provided with dark pre- and post-exposure fields. For similar reasons the thoroughgoing experimental work of Duerr[2] is not directly

[1] Helmholtz, *Physiologische Optik,* 2 ed., p. 480, p. 488, fol. Aubert, *Physiologie der Netghaut,* p. 96.

[2] *Phil. Studien,* Vol. 18, p. 215, fol.

relevant to our present enquiry. Nevertheless the tachistoscope offers the best conditions for the solution of our problem, provided we realize its limitations. While the problem itself throws new light on tachistoscopic procedures.

§ 3. *Tachistoscopic Exposure.*

The most satisfactory exposure time in tachistoscopic experiment is doubtless not a constant but an experimental variable, whose value in any given experiment must be determined by experimental tact and expediency. The effect of its variation, on the other hand, is not a matter of tact or expediency. It is a definite psychophysical problem, capable of scientific analysis. In view of the extensive use of the tachistoscope, particularly in the study of the psychology of reading, where a study of fixation finds its most immediate practical application, and in view of the marked differences in the usage and findings of various experimenters, as well as in view of the use we must make of the tachistoscope in the studies of our more immediate problem, an analysis of the general conditions of tachistoscopic exposure needs no apology. If I may state my conclusions at the beginning, I find that the tendency to reduce the physical exposure time to a minimum wherever the aim is to present an adequate exposure of the simplest kind is a methodological mistake, based on a psychophysical misconception. It introduces unusual conditions altogether foreign to the natural fixation pause and leads, or may well lead, to a distorted analysis of the processes of apprehension; making the conclusions, in so far as they are referred to normal perception, not merely valueless but false. Obviously my thesis does not refer to those tachistoscopic experiments which aim to measure retinal inertia or the legibility of letters by the determination of the threshold exposure. In such experiments minimal exposures are the essential part of the experiment and are obviously justified. My contention is merely this, that visual perception from a threshold exposure may be and indeed must be quite a different matter from normal visual perception, and that the results of the former should be applied to the latter only where there is clear justification of the analogy.

Doubtless the main motive for working with a minimum physical exposure time is to reduce the stimulus and the consequent psychophysical process to the simplest possible terms. The experimenter seeks to isolate a single apperceptive event—to eliminate if possible all changes in the position of the visual point of regard, and in the direction of attention. Even if complete simplification of these central and peripheral elements is impossible the experimenter expects to simplify them as far as possible, to retard the processes involved, and to render them more susceptible to introspective observation. Both the means and the end are legitimate enough in themselves, our only question concerns their relationship. The immediate critical question is: does a threshold exposure simplify the consequent psychophysical process or render it more apparent to introspection. In the first place it should be noted that a minimum exposure is not necessary to eliminate reactive eye movements. As we have already seen, an average reactive eye movement to a change in the visual field is well over 150 σ. In view of the fact, however, that the eye is never absolutely at rest even during the sharpest fixation, there still appears to be some peripheral motive for a minimum exposure, to reduce to a minimum the effect of the minute fixation movements. On the other hand, the normal fixation movements are relatively slow and of relatively small amplitude. Especially is this true, if we eliminate the necessity for a new adjustment of convergence, by providing an adequate pre-exposure fixation mark. Under these circumstances an exposure interval of .1″ guarantees what is physiologically a single visual act.

Much more significant however for the reduction of the exposure time below .1″ is the psychological motive. There are no satisfactory experimental guides for the rapidity of changes of attention. There is no reliable measurement of the minimum duration of an elementary apperceptive process. The natural tendency therefore is to reduce the exposure time to the lowest practicable limit, with the express or implied expectation that the psychological processes are thereby reduced to the lowest terms. Such for instance is the motive

of the low exposure times in the tachistoscopic work of Zeitler[1] and of Messmer.[2]

This procedure, however, involves two fundamental fallacies. (1) It is not necessarily true that a low exposure time guarantees a minimum physiological excitation. Any given physical stimulus depends for its physiological consequences not only on the time it is allowed to act, but also on the pre- and post-exposure conditions, viz., on its clearing up. (2) It is never true that the complexity of the psychological consequences varies directly with the intensity of the physiological excitation.

It is noteworthy that the apparent duration of the exposure by the electric spark does not differ materially from the apparent duration of an exposure of .1″ under proper conditions of illumination. Similarly, with adequate changes either in the absolute illumination, or in the relation between the illumination of the pre- and post-exposure fields, an exposure of .1″ may have the same apparent duration as an exposure of .001″. Changes in the exposure time are commonly apprehended as changes in the amount of illumination; and conversely, changes in illumination are often apprehended as changes in the duration of the exposure.

It is further noteworthy that the threshold duration of exposure is a variable, dependent on the same conditions that determine the apparent duration of exposure, viz., on the absolute illumination of the exposed object and on the relative illumination of the pre- and post-exposure fields. An exposure time of 30 σ may be and is as truly a threshold exposure if the illumination of the pre- and post-exposure fields equals that of the exposure field, as is an exposure of 1 σ when the pre- and post-exposure fields are black. Moreover, there are excellent grounds for believing that the threshold exposure in the former case may represent a total physiological excitation in no wise longer than that in the latter case. Even if a threshold exposure time is desirable, then, I contend that an exposure time of 1 σ can never be used without artificially prolonging the

[1] J. Zeitler, 'Tachistoscopische Versuche über das Lesen,' *Phil Studien,* XVI.
[2] O. Messmer, 'Zur Psychologie des Lesens,' *Archiv. f. d. Gesammt. Psy.,* II.

positive after-image by the use of a dark post-exposure field. To insist that one must have an exposure interval of 1σ in order to get a minimum physiological effect, without taking pains to obliterate the physiological after-effect, is to say the least inconsiderate of a considerable body of our best traditions.

My second contention is that anything approximating a threshold exposure, instead of simplifying the consequent psychological process, really complicates it and renders it more uncertain. This is indicated, first of all, by the most obvious results of reaction experiments. The minimum exposure of a word increases the speech reaction time indefinitely to infinity. We may well use the results of reaction experiments with caution, but, if they mean anything at all, a delayed reaction of two seconds under otherwise similar circumstances must involve a more complex central process than one from 300–400 σ. Unless we surrender all commonsense to the. scientific abstraction of a differentiated apperceptive and assimilative process, it is absurd to maintain that increased reaction time represents simplified mental processes. My contention is supported further by the familiar facts of normal perception. It is not the clear and distinct visual impressions of bright sunlight that suffer the most 'assimilative' distortion and falsification. It is those impressions which just differentiate themselves from their background, whether on account of defective illumination, deficient size, or minimal exposure that lend themselves best to the vagaries of subjective interpretation. The illusions of the dusk are difficult to understand at noon. Analogous errors attend the use of minimal exposure intervals in reading. Words are more freely misread; letters are substituted, inserted, or omitted altogether with characteristic frequency. Or on the other hand for more careful observers, whose attention is directed primarily to the letter content of the exposure, minimal exposure emphasizes that part of the field that falls on the macula. But the real question is not whether we have to do with more or less complicated mental processes in these characteristic falsifications and selections. The question is whether an introspective analysis of perception under these unusual conditions, however valuable it may otherwise be, will

also constitute an analysis of normal perception. Obviously enough some new disturbing elements have been introduced into the process, which give rise to the increased uncertainty of the perceptual process; and the tachistoscopic experimenter must be on his guard not to pick out exactly these new disturbing factors and discover in them the otherwise hidden elements of normal perception. This, however, is exactly what Zeitler[1] does in his successive apprehension hypothesis. The subsequent detailed review of the memory image of an inadequately cleared-up word is without question a successive process. It is, however, produced by the experimental conditions not disclosed by them. The old scientific battle-cry of 'empfundene Empfindungen' may well be paralled by a new one of experimentally manufactured disclosures. Messmer[2] repudiates the successive apprehension hypothesis of Zeitler and proposes a substitute successive apprehension hypothesis, dependent on the difference of legibility of the letters. Dominant, or in general large letters are held to come to consciousness first.

Even more obviously than Zeitler's, Messmer's appears to be an experimentally manufactured disclosure. It is familiar to every experimenter with tachistoscopic exposures, since the work of Exner, that the threshold exposure varies not only with the illumination and the length of the after-image, but also with the size of the exposed objects. The threshold exposure tends to isolate the larger letters or to exaggerate them —exactly as Messmer and his keen observers find to be the fact. But the exaggeration is a product of his experiments, not their discovery. The apparent selective emphasis of the larger letters in a group is a beautiful experiment in threshold exposures.

In support of Messmer's contention it may be urged that the temporal advantage of the large letters discovered by the tachistoscope is a real one, even in the longer exposures of normal reading. Namely, in imagination at least, each full reading pause of from 100–500 σ must have had at its beginning a moment when the threshold stimulus with all its selective emphasis was approximated. This is Messmer's own

[1] 'Tachistoscopische Versuche über das Lesen,' *op. cit.*

[2] 'Zur Psychologie des Lesens,' *op. cit.*

contention; but it rests on a false hypothesis of the development of visual excitation. Threshold stimuli are much less rapid in their physiological development than stronger stimuli, and it is inevitable that before the excitation produced by the first moment of stimulation could have fully developed it must be overtaken by the excitation produced by the subsequent adequate stimulation. My real contention after all is against the attempt to transfer the effects of minimal exposure to the normal processes of apprehension. Conversely, I believe that the only exposure, whose results will apply directly to normal processes, is that which, under the given experimental conditions of illumination, will permit a full and uniformly cleared-up visual impression. For this exposure I believe we still have a right to the superlative implied in the word 'tachistoscopic,' since we refer not to the most rapid excitation but to the most rapid *vision* which is really cleared up and adequate. To discover what duration of exposure will guarantee such vision is a part of our study of the clearing-up process.

§ 4. *Influence of the Pre- and Post-exposure Fields.*

Two general demonstrations of the influence of the pre- and post-exposure fields on the clearing-up process may serve as an introduction to the more detailed investigation.

Experiment 1.—If an even white field is exposed on a pre- and post-exposure field half white and half black the minimum duration of the exposure that conditions the perception of an even white field will be the physical condition of a maximum clearing up of the white field, which for obvious reasons is not to be confused with the maximum excitation. With whites of moderate intensity the threshold of exposure will lie below 1 σ. With a threshold exposure, however, the exposed white field is not seen as white at all. It barely distinguishes itself from the black pre- and post-exposure field, as a gray. The minimum exposure is incapable of giving a cleared up impression of the white object. Indeed as compared with the neighboring white which remains constant during the entire experiment, a maximum clearing up is only accomplished by an exposure of from .5″ to several seconds, according to the duration of the pre-exposure fixation.

Experiment 1.

Pre-exposure		Exposure	Post-exposure	
white	black,	white,	white	black.

Experiment 2.—If the whole pre-exposure field is black, while the exposure is uniform gray and the post-exposure field is white an exposure of 50 σ may utterly fail to reveal any trace of the gray. That is, the gray exposure, a duration of 50 σ, is lost in the clearing up of the white post-exposure field.

Experiment 2.

Pre-exposure	Exposure	Post-exposure
black	gray	white.

Frappant and suggestive variations of this second experiment may be made with colored exposures on colored pre- and post-exposure fields. These variations seem to offer striking demonstrations of the degree of relationship between colors. Accurate results, particularly with respect to introspection, require some form of exposure apparatus giving simultaneous exposures of the entire exposure field, with full control of duration and illumination. The only apparatus guaranteeing satisfactory conditions, in which there is full control of the illumination of the pre- and post-exposure fields, and the possibility of wide variations in the length of exposure without changing any other of the conditions of exposure, is the transparent mirror apparatus.[1] Plate II. shows the tachistoscope in position. The ordinary fall exposure apparatus is particularly objectionable for use in these experiments because of the width of their exposure field, and the obvious passing of the shutter edges in long exposures.

The second experiment seems to me of especial interest, in view of a recent discussion of the illusion of clear vision during eye movement. The demonstration that a fusion of diverse stimuli may be exposed 50 σ without clearing up, when the pre- and post-exposure fields consist respectively of the colors which are combined in the fusion exposure, is directly analogous to the ordinary lack of perception of the fusion of the field of view during short eye movements.

[1] Psychological Bulletin, Vol. IV., pp. 10–13.

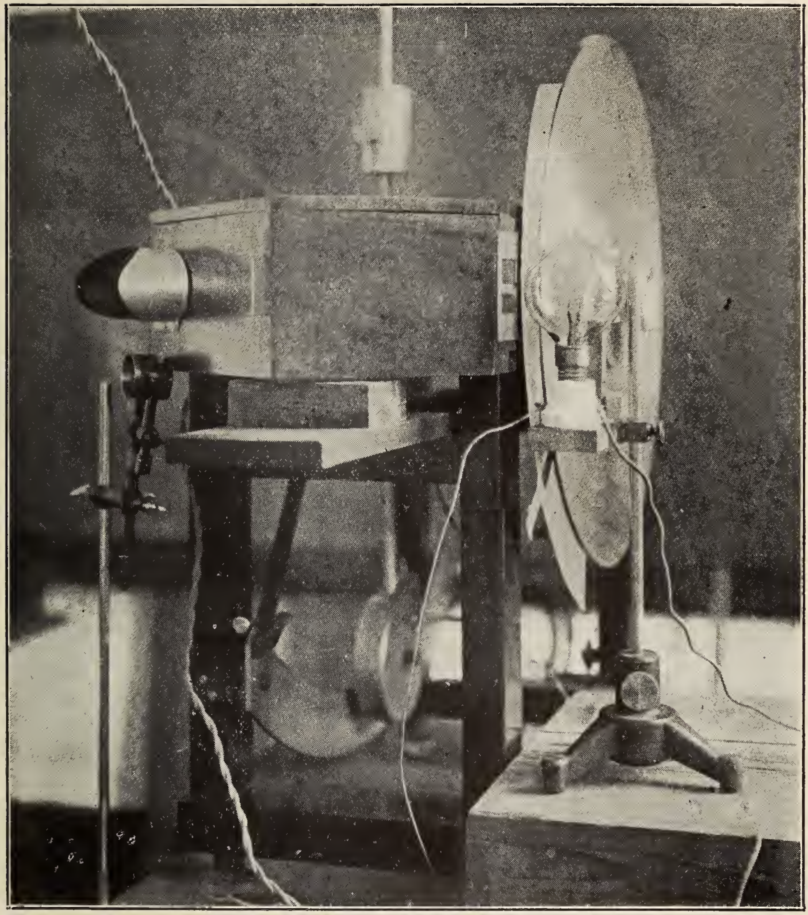

Plate II.
The Transparent Mirror Tachistoscope and Pendulum Shutter.

While these experiments clearly if roughly indicate the influence of the pre- and post-exposure fields on the clearing up of an exposure, closer experimental determination of this influence is limited only by the theoretical and practical interests involved. Partly from obvious ulterior reasons of special interest, and partly because I believe that the psychology of reading offers exceptionably favorable experimental material for the study of the general processes of visual perception, the following, more detailed inquiry is largely based on tachistoscopic reading.

For reasons already given the apparatus was my binocular transparent mirror tachistoscope. The shutter was a heavy second pendulum with concentric disks attached to its axis. Exposure under these circumstances was noiseless, and might occur isolated or in groups of any prearranged number at intervals of one second.

Starting with an equal illumination of the pre- and post-exposure fields, words of the style of type used in the PSYCHOLOGICAL BULLETIN, exposed for a total legible interval of 20 σ (10 σ of which was maximum illumination) were seen as illegible faint gray masses. Succeeding exposures at intervals of 1″, occurring as the pendulum was allowed to swing back and forth, made an examination of the various parts of the gray word masses possible but they utterly failed to clear up as wholes. Even when a word was suggested, that seemed to correspond to the general appearance of the gray mass, there was no corresponding general clearing up. In successive exposures, however, two or three letters seemed to flash out somewhat more distinctly from the gray mass. These selected letters were not always together but they seemed to be at or near the point of regard. While they were not always the so-called dominant letters, the latter were conspicuously apt to appear. The phenomenon is in direct agreement with Messmer's keen observations. In the present instance, however, it was clear that the cause was a differential threshold for the larger objects lying near the point of regard.

Three variations of the second experiment gave sufficiently cleared-up impressions to make the words legible. The words

became legible if they were increased in size, though the differential emphasis on the larger letters still remained. Words of the same sized letters were legible if the exposure was increased to 30 σ; or again, without changing the exposure time, if the relative illumination of the exposure field was slightly increased.

These results remained identical for numerous series of control experiments made at various times of the day and night, and extending over a period of a year and a half. Unfortunately no one of sufficient training was available for a complete series of control experiments. But a number of partially completed series made it clear that the initial subthreshold exposure time of 20 σ was of individual value. All the inexperienced subjects demanded a longer exposure. Otherwise the above findings seem to have general validity.

The differential emphasis on the larger letters has already been considered. The differential illumination of the exposure and the pre- and post-exposure fields demanded further investigation. It was impracticable with my present apparatus, without changing the form of the shutter, to so far reduce the exposure time as to reach the threshold exposure with a dark gray pre- and post-exposure field. That has already been accomplished by Exner, Cattell, and others. On the other hand, the relative inhibition of dark and light pre- and post-exposure fields to the clearing-up process was directly in line with our main inquiry.

Experiment 3.—Following up this special problem I divided the pre- and post-exposure fields into a dark and light area, as in experiment 1, by covering half of the field with a piece of ordinary commercial black bristol board. For the exposure object I took the phrase 'week after week.' The line of division between the light and the dark part of the pre-fixation field passed through the 't' in after. Starting with an exposure time of 20 σ, I increased it by gradual stages to 500 σ carefuly noting the variation in the appearance of the two halves of the exposure object. At 20 σ only that part of the phrase that fell on the black pre-exposure field was sufficiently cleared to be legible. The word appearing on the

white pre-exposure field was only a faint gray mass, as in experiment 2. As the exposure time gradually increased the word on the black field became clearer and sharper, while the previously illegible word on the white field gradually cleared and became darker. At no time was there a sharp transition between the appearance of the words in successive exposures. Not until the exposure time reached 120 σ was it possible to speak of a full clearing up of the word on the white field. With that exposure time, the letters on the white field, and their background on the black field, seemed to clear up naturally. The only word whose legibility was at all disturbed at that exposure time was the word 'after,' half of which it will be remembered fell on either field. In spite of the fact that both sides seemed to clear up there was still a marked difference between them. There was a marked difference in the apparent time of appearance and disappearance, and there was persistent difference between the blackness of the letters of the two words and the whiteness of their respective grounds. These differences persisted, though to a less marked degree, as the exposure was gradually lengthened to 500 σ; beyond which the experiment was not carried. The results of experiment 3 show that an *absolute* clearing up of an exposed word does not occur even with an exposure of 500 σ. The length of exposure giving an adequate clearing up varies with the illumination of the pre- and post-exposure fields. The greatest inhibition arises from a combination of light and dark elements in the pre- and post-exposure field of a single word.

Experiment 4.—The question whether the pre- or the post-exposure field exercised the greater inhibition on the development of the clearing-up process is capable of experimental investigation.

Again using the same arrangement of exposure apparatus and pendulum shutter, a set of experiments was made under varying exposure time with the following arrangement. During one half of each swing of the pendulum the pre- or post-exposure field was completely darkened. This gave the following arrangement at each complete oscillation of the pendulum:

EXPERIMENT 4.

Pre-exposure	Exposure	Post-exposure
out—black	word	white
back—white	word	black

That is to say, each double swing of the pendulum gave two full exposures of the word, preceded by black and followed by white on the out swing, and preceded by white and followed by black on the return. If the exposure time was reduced to 30 σ, which was the minimum exposure giving a clearing up to the point of legibility, a curious phenomenon was observed. Each swing of the pendulum in the direction black, ex., white gave a legible exposure. Each swing in the opposite direction, white, ex., black gave an illegible exposure. This was directly contrary to expectation, but the fact was unequivocal. Observations with increased exposure time corroborated the previous results. The exposure in the direction black, ex., white was regularly clearer, as the pendulum swung back and forth, than the exposure in the direction white, ex., black. The results were independent of the position of the point of regard, and no voluntary control of the attention could change them. On the other hand, when the exposure field was made brighter than the white pre- or post-exposure field, the reverse direction became more favorable. This gave the clue to a plausible explanation of the phenomenon.

Assuming in the first instance that the black of the letters equals the black of the pre-exposure field, and that the white of the letter background equals the white of the post-exposure field, then the progress of the exposure, black, ex., white, conditions a clearing-up process as follows: The letters as they appear are a direct continuation of the black of the pre-exposure field, and they stand out in relief as the pre-exposure black passes through gray into the cleared-up white of the post-exposure field. Through the entire clearing-up process the letters stand out darker than their surroundings, on account of the initial delay in the clearing up of the white, which they condition. In the reverse order from white to black, since the entire black stimulus is apparently shorter than the white, the duration of the slight differential advantage in the clearing up of the black, corresponding to the letters, will be shorter;

and there will consequently be an exposure time when this virtual shortening of the effective stimulus will reduce it below the threshold. When, on the other hand, the illumination is such that the exposure is brighter than the white or the post-exposure field, the letters also become lighter than the black of the pre-exposure field. They will consequently tend to perpetuate themselves into the black post-exposure field as lighter elements, while they become less distinguishable from the simple clearing-up gray, in passing from black to white.

The experiment indicates how complex the conditions of the clearing-up process really are. In general, however, one can say that the relatively slight differences between the two arrangements of the black and white fields indicate that, the other conditions being equal, the influence of the pre- and post-exposure fields on the clearing-up process are approximately equal, even when their mode of action is different.

In a measure, all these experiments, with relatively simple pre- and post-exposure fields, present unnatural or unusual conditions rarely or never found in the succession of fixations in normal vision. In most instances in normal vision the visual field antecedent to, as well as that consequent to, any given fixation will be of approximately the same complication as that of the selected fixation. Now we already have some evidence that the complication of the pre- and post-exposure fields delays or inhibits the clearing up of the exposure. Direct experiment discloses an irregular variation of the clearing-up process consequent to a regular increase in the complication of the pre- and post-exposure fields.

Experiment 5.—A single horizontal line, so placed in the pre- and post-exposure field that a word is exposed directly where the line was, will render uncertain and indistinct the otherwise adequate exposure of 30 σ. Several additional horizontal lines, however, so oriented as to occupy the entire area to be covered by the exposed word fail to increase the disturbance. Indeed on a background of five horizontal lines covering the entire area of the word, the latter appeared not only plainer than with the pre- and post-exposure complication of a single line, but it was clearer than without any complica-

tion at all. A group of vertical lines, on the other hand, spaced about the same as the letters, almost completely obliterated the word. Only occasionally, and then after a succession of exposures, was there a suggestion of word form or individual letters. Not until the exposure time was increased to 50 σ did the clearing-up process approximate that which was conditioned by an exposure of 30 σ on a plain white pre-exposure field. The paradoxical character of these results is increased by the close observation of a word clearing up from a complicated pre-exposure field, during a long exposure. It was very clear that a dark line on the pre-exposure field really facilitated the clearing up of parts of the letter that crossed it. A still more serious interference with the clearing up of a word was found to be the complication of the pre- and post-exposure fields by an irregular tangle of lines. But again it was found that too great complication entirely reversed this result. The probable explanation of the decrease in the effect of too highly complicated fields is that the extreme complication acts like an even gray. The most pronounced interference with the clearing-up process of an exposed word that I was able to produce, without decreasing the illumination of the exposure, was by a pre- and post-exposure complication of exactly the same general character as the exposure, *i. e.*, by a word.

Experiment 6.—The following table shows the effect of pre- and post-exposure complication by a mirror image of the word 'explanation.' Each word was exposed in two ways, lettered, respectively, *A* and *B*. *A* is the result of the first single exposure. *B* is the result of a series of several exposures, succeeding each other at intervals of 1″. *T* gives the total time of legibility, which is regularly 10 σ longer than the time of maximum illumination.

$T = 48\ \sigma$.

1, Substance, *A,* No clue to what the word might be.
B, The word substance came to mind without conscious preliminaries, as a sort of inspiration. It was incapable of verification in successive exposures.

2, Paragraphs, *A,* Nothing.
B, The word paragraphs came in the same way as in No. 1. It was incapable of verification in subsequent exposures.

3, Verity, *A,* Nothing.
B, The word reality came as above. Subsequent exposures verified only the *ty.*

4, The articles, *A*, Impression of two words.
B, No inspiration.
5, Accuracy, *A*, Nothing.
B, 'Possibly emotion.'
6, In, *A*, Nothing.
B, 'In, doubtful,' not verifiable.
7, Extended, *A*, Nothing.
B, Nothing.
8, Suggestion *A*, Nothing.
B, 'Suspicion' not verifiable.

$T = 70\,\sigma$.

Same complication of pre- and post-exposure fields.

1, Fundamental, *A*, 'Fundamental' tentative
B, Successive exposures do not give cleared up impression.
2, Discussion, *A*, Nothing.
B, 'Discussion' verifiable on successive exposures.
3, Degree, *A*, 'Degree very plain' (the word was half a letter too low).
4, Thought *A*, 'Thoughts.'
B, Failed to clear up.
5, Judicious, *A*, Jealous.
B, Judicious.

As the time was reduced below $70\,\sigma$ the inadequacy of the exposure became more and more obvious. But even $70\,\sigma$ was obviously too short for an adequate clearing up. An increase of the exposure time under otherwise identical conditions gave the following results:

$T = 170\,\sigma$. Unnecessarily long, fully cleared up.
$T = 125\,\sigma$. Fully cleared up appearance. No ambiquity. Every word read correctly and without hesitation.
$T = 80\,\sigma$. Scarcely better than $70\,\sigma$.
$T = 100\,\sigma$. No mistakes but the words do not fully clear up. The exposure seems uncomfortably short.

Notable differences resulted from changing the size of the letters of the exposed word. With the same complication that made a 12 pt. word illegible when $T = 70\,\sigma$, a 24 pt. word was entirely legible and fairly cleared up, and even a 16 pt. word was legible.

§ 5. *Normal Interference of Successive Fixations.*

Obviously none of these experiments quite corresponds with normal vision. The complication of two normal fixation fields

is seldom identical; and secondly, every natural fixation pause is preceded by a moment of inadequate neutral stimulation of from $15\,\sigma$–$100\,\sigma$ duration (the duration of the eye movements) during which the after effect of the pre-fixation field is somewhat abated; while the same period at the end of the normal exposure must elapse before the operation of the post-exposure field. Obviously some control was necessary before the results of the previous experiments could be transferred to normal vision. To that end I constructed an escapement exposure apparatus, in which each new exposure was produced by the rapid movement of the words into place behind a narrow slit. This movement of the field of view is the best available experimental substitute for the moments of inadequate retinal stimulation due to eye movements. The escapement exposure apparatus makes a fairly satisfactory arrangement for the exposure of any small objects.

In the form used by me, the apparatus consists of a light wooden framework, carrying a screen towards the observer, which covers the mechanism and the word-disk with the exception of the one word which is being exposed. Behind the screen is a light metal sector which carries the word-disk. Equally spaced teeth on the periphery of this sector engage an escapement so that whenever the free end of the escapement is depressed one and only one tooth may slip past. As the words on the word-disk have the same angular distance as the teeth on the periphery of the sector, each depression of the escapement brings a new word into place before the aperture in the screen. The time of exposure is determined by the rapidity with which the escapement is depressed. The whole apparatus was bent up from sheet tin, and works admirably. The springs actuating the various parts were empirically adjusted until there was no apparent vibration of the word, as it came into place; and until there was the least possible suggestion of motion as each new object was sprung into position. The escapement was actuated by adjustable teeth, attached to the disk on our second-pendulum. Since these teeth could be of any number and any desirable distance apart, provided the entire series did not occupy more than $750\,\sigma$, the number and

duration of the exposures was capable of considerable experimental modification. A tuning fork record near the periphery of the disk permitted rapid experimental variation of the exposure. The results with the escapement apparatus showed that the results of experiment 6 are applicable to normal conditions with comparatively slight correction. An exposure interval of 50 σ gave four correct readings out of ten. In one of the four the sequence was inadvertently known. In all of them the number of possible words was known. In all four the same curious phenomenon was observed that was described in experiment 6. The word came to consciousness as a sort of inspiration, apparently without any real justification in the observed facts. This same experience was confirmed by a number of visiting psychologists and is rather suggestive, not only on account of its relation to experiments to be described later in connection with peripheral vision, but also because of its relation to the current discussion of the subconscious. There are numerous familiar mental phenomena that suggest a similar explanation of origin in partially cleared-up sensory impressions, but there are comparatively few that so lend themselves to direct experimental analysis.

An exposure time of 60 σ permitted correct reading of all the words provided only one was exposed at a time. On the other hand an exposure interval of 100 σ was necessary to get a fairly cleared-up impression, and this was the approximate threshold for a succession of 3–4 exposures.

Notwithstanding the general agreement with the previous experiments, the results indicate that the more natural conditions of exposure involving a neutral inter-exposure interval, and different pre- and post-exposure fields have a real if a relatively small advantage over the conditions obtaining in the transparent mirror tachistoscope. They add to our previous discussion the new fact that in a succession of equally timed exposures the threshold of adequate exposure is materially raised; and conversely, that the relative completeness of the clearing-up process becomes more and more imperative. With equally illuminated and equaly complicated fields, as, for example in reading, 80 σ to 100 σ is the minimum adequate exposure.

Two further control experiments completely justify this conclusion. It is possible to make a voluntary sweep of the eye from a given fixation point out and back, so rapidly as to preclude all clear and distinct vision during the eye movement. The operation may require some practice. Similarly it is possible to make a succession of eye movements in a given direction so rapidly as to prevent clear vision during the movement. In cases where the field just clears up in the former case there is regularly an illusion of motion.

It was not difficult to obtain a photograph of the eye movements under these conditions. While a detailed examination of the record would be of little value since none of the fixations are even relatively good, the record shows that real fixation pauses may be too short to give a cleared-up impression. Further data in the matter more immediately capable of interpretation is found in the records of the most rapid possible reading (Fig. 2), which are discussed later more in detail. Huey and Dearborn both find reading pauses well below the recorded reaction time of the eyes. This is clearly confirmed by my records. The fact suggests a considerable complication of the visual processes which constitutes the central problem of our third chapter. Our immediate interest in the facts abstracts from the complex apperceptive process and fixates the immediate visual pre-conditions. Obviously it is really impossible to separate the two factors. No reading process can exclude the reproductive processes which are the universal condition of developed perception. Indeed their presence is implied in every judgment of an adequate fixation. But while the experimental elimination of this factor is impracticable, its temporal effect on the reading pauses may at least be reduced to a minimum by complete familiarity with the text and by the effort to read with the utmost rapidity. Even under these circumstances the shortest adequate fixation pauses in reading are between 70 σ and 100 σ. See Fig. 2. And these are not infrequently accompanied by a feeling of dimness. With the shortest fixation pauses there is conspicuous lack of a full clearing up. While accurate records are probably not available to most of my readers the ability to move the eyes faster

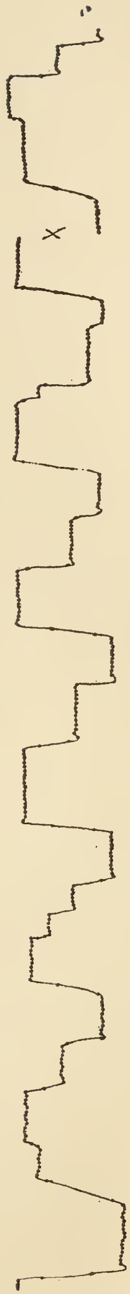

FIG. 2.

RECORD OF THE EYE MOVEMENTS AND FIXATIONS IN READING.

FIG. 2 is a drawing one and one half actual size, from a record of the eye movements during the most rapid possible reading of several lines of familiar text. The record should be read from below up. Vertical lines correspond to fixation pauses. Oblique lines correspond to eye movements. The drawing was made by projection and represents fairly accurately the appearance of the record except that the dots appearing in the oblique lines should be much fainter and much elongated.

The waviness in the early fixations is instrumental and resulted from a jarring of the apparatus.

The absence of head line makes it impossible to demonstrate accurately the meaning of some of the lesser fixation movements, but the particular series of investigations which these plates subserved was temporal and not spatial. Each dot represents a complete phase of the alternating current—and approximately 1/60″.

Fixations 1 and 2 are preliminary control fixations, respectively, at the beginning and end of the line.

The return from 2 to the beginning of the line is interrupted by a relatively short fixation too far along in the line; a corrective movement brings the extreme end of the line into view.

The first two lines have four fixation pauses each, showing the same tendency which Dearborn noted to make more fixations in the first part of a passage.

The drawing clearly shows the little hooks at the end of the long sweeps, as well as at the end of many of the shorter sweeps. Huey first called attention to these overshoots, but for some time it seemed doubtful whether they were real eye movements or were due to the momentum of the recording lever. The less delicate lines of my earlier photographic records failed to show them, but they appear to be regular characteristics of my reading records. I have counted them as a part of the fixation pause, but this procedure is open to serious question.

The shortest fixation is fixation 2, line 5. It lasted cr. 40 σ.

than the print clears up will be capable of easy proof. The form of the eye movements under which the phenomenon occurs must be taken from the available records.

Both experimentally and practically the operation of the visual clearing-up process precludes a succession of adequate visual fixations under .1″ each.

Chapter III. The Complication of the Visual Processes During Fixation.

§ 1. *Eccentric Vision in Reading.*

The records of reading from which at the close of the last chapter we drew illustration and corroboration of our generalization concerning the minimal clearing-up process, correspond as poorly with the available data concerning ocular reactions as with the results of reading reactions in general. It will be remembered that my reaction time for ocular movement in response to a peripheral visual impression is 165 σ. This is complicated in reading at least by the complex apperceptive processes which condition the apprehension of the words as such, their contribution to the sum-total meaning consciousness, and the more or less definite mental and physical reactions to that meaning. There would be obvious difficulty in crowding all this into a period of 100 σ. But the utter absurdity of the attempt to crowd it into the minimal pause of 40 σ relieves us of the obligation. We are forced to postulate a concurrent complication of the psychophysical processes of perception, extending through several fixations. The facts demand an attempt to submit the whole tangle of concurrent processes to experimental analysis. But the main question of our present discussion is the relation of the comparatively short moments of clear vision to the total perceptual process.

It is a significant fact, first noticed by Dearborn, that the initial fixation in reading a passage tends to be relatively long. My own records, as illustrated by Fig. 2, confirm his generalization. But in view of the probable complication of the visual processes, it seemed worth while to enquire more specifically just how long the initial fixation will be when a group of words suddenly appears at or near the area of clear vision, without the usual peripheral and conceptual, pre-fixational preparation for its perception. Such an experiment should furnish reliable data for determining where in the complex

elaboration of the sense material the normal fixation pause really belongs.

To that end the recording apparatus was arranged substantially as illustrated in Fig. 1, and Plate III., for the ocular reaction experiments. The falling screen shutter exposed a word or group of words simultaneously with the opening of a path for the recording beam of blue light. The mouth bit was placed in a brass tube whose far end was covered by a rubber diaphragm. The slightest vibration of the latter would break an electric circuit which would in turn release a screen, shutting off the light, and thus ending the record. These records have the same general appearance as the enlarged reaction record, Plate I., Fig. 3, except that the whole record is in some cases much longer. When two words were exposed sufficiently separated to necessitate a separate fixation act for each word, the fixations of the first word are universally somewhat shorter than the voice reaction to that word. The eye is through with the first word and has gone on to the next before the first word is spoken. The exact relations in a typical record are as follows:

From the exposure to the fixation of the second word, two inches to the right of the first	722 σ
From the exposure to the voice reaction of the first word which is four in. from the primary fixation mark	779 σ
The voice reaction to the first word after fixation	365 σ
The voice reaction to the second word after its fixation	357 σ

The first two values indicate that the two processes, the elaboration of the speech reaction and the preparation of the eye movement necessary to fixate the second word, are proceeding simultaneously. The speech reaction is only 57 σ later than the end of the fixation. In other words the first word was spoken after the second had been fixated for a considerable length of time. The records made under these circumstances are only four in number, but all agree in the points above stated. Few as they are they furnish confirmation for Dearborn's hypothesis to explain the relative length of the first fixation in a line; viz., that the eye in the first fixation takes a more general survey of the line as a whole than it does in the latter fixation.[1] But the records indicate that this general

[1] Dearborn, *Psychology of Reading*, p. 63.

survey is of much greater importance in the general reading process than most of us had ever imagined. The attempt to obtain some quantitative estimate of the importance of this pre-fixational vision led to the abandonment of the experiment in the above crude form and its continuation under more exact conditions to be described presently. Finally the records seem to indicate that the normal fixation pause in reading corresponds to the latter part of the unprepared fixation of our experiment. It seems probable that the normal reading pause represents a comparatively late moment in the total process of perception of the fixated object. If this be capable of substantiation, it must cause a considerable increase in the traditional estimation of the relative importance of peripheral vision.

The extent of this area of extra-foveal pre-perception together with the relative value of the pre-fixational data from different zones of the retina is capable of experimental determination. To this end I arranged the following modification of the above described preliminary experiments.

Instead of exposing the word directly at the fixation area, each word was placed at some predetermined distance from the fixation point. In each case this necessitated a reactive eye movement before the exposed word was brought to the area of clear vision. In each case on the other hand there was a pre-fixational vision corresponding to the normal reaction time of the eye. The relative effect of this comparatively constant pre-fixational vision, according as it involved different zones of the retina is the point of interest in the experiments and is given diagrammatically in the following figures.

These experiments indicate that the pre-fixational perception is in no wise limited to the immediate neighborhood of the macula. It may, on occasion at least, extend clear across the length of an 8vo line; and there is some evidence that it is not confined to the specific line which is ostensibly being read. The possibility of intelligent voice modulation in reading aloud from totally unfamiliar text is not wholly due to the advanced position of the eyes in vocalized reading; it also indicates the breadth of the extra-foveal pre-perception. While the well-known disturbances caused by right-sided hemianopsia indicate its necessity in all normal reading.

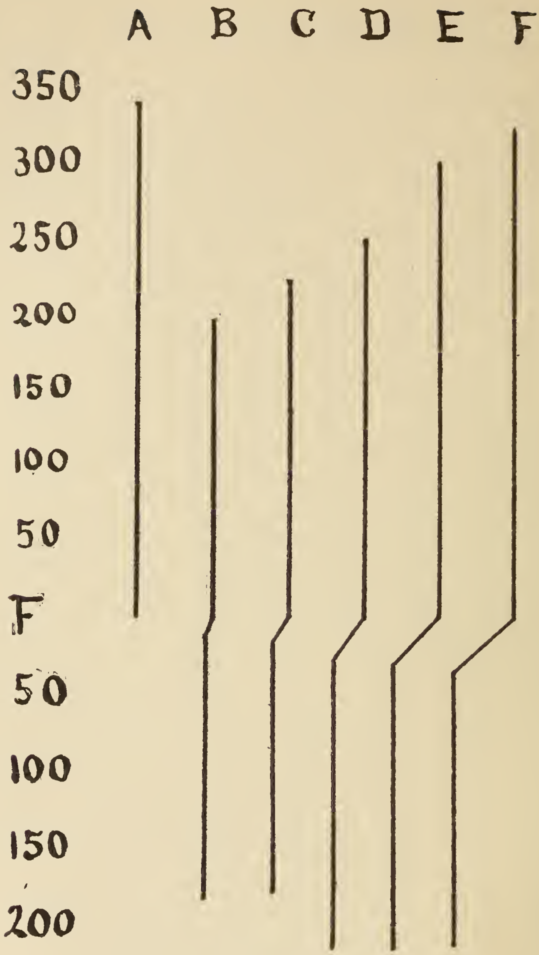

FIG. 3.

Line *A* = the average duration of a speech reaction in thousandths of a second, when a four-letter word was exposed at the fixation mark. 21 records—all included.

Line *B* shows the influence of the pre-fixational exposure in shortening the speech reaction when the word is exposed 1/2 in. to the right or left of the fixation mark. 12 records.

Line *C* shows the same effect, slightly less, when the word was 1 in. to the right or left. 6 records.

Line *D* shows the saving when the word was 2 in. right or left. 7 records.

Lines *E* and *F* show the saving when the word was respectively 3 in. and 4 in. to the right of the fixation mark. 6 and 3 records, respectively.

I venture the belief that the above demonstration of peripheral complication of the visual processes in reading is capable of the widest generalization. Nothing is isolated in the visual field merely because it is situated at the fovea. Every fixated object, even in tachistoscopic exposures is seen in a more or less complex setting. And the setting of each fixated object must be given by more or less complex extra-foveal data which are elaborated in complication with the data of foveal vision. Somewhere within the concurrent processes arise the motives for new fixations and the processes of abstraction by which alone we may speak of individual objects of regard within the visual field. What or how large the perceptual unit shall be is, sensorily considered, a matter of complete indifference. The perceptual unit is determined by variable concurrent central processes whose complex conscious correlates are expressed by the terms interest and attention.

§ 2. *Successive Elements of the Process of Perception.*

To speak of the perception of a word in ordinary reading as a successive process seems to be altogether in harmony with the facts. In general it would be difficult to conceive of a *process* in which there was no succession, and we have demonstrated experimentally that the perceptual processes in reading may begin one or more fixations before the specific word is directly fixated—if indeed it ever is fixated. But to insist that this succession in the perceptual processes necessarily involves a successive apprehension of letter units seems to me as absurd as would be the contention that we cannot perceive a house without a successive apprehension of the clapboards and shingles at least the 'dominating' ones. To insist on the other hand that such a successive apprehension of the letters is impossible would be equally absurd, provided they fell within the area of relatively clear vision. But the mental clearing up of these relatively minute perceptual units will depend on definite concurrent central complications in which these particular units are the objects of interest and attention. One may very readily demonstrate the effect of such central complications by suitable tachistoscopic experiments. But to contend

that the experimentally manufactured facts are a phenomenon of ordinary reading is as futile as to contend that the process of the perception of words would be explained if one could say that words are perceived by a successive apprehension of their parts. The real problems of perception, the determination of the units of the perceptual process, the concurrent perception of groups, the relation of the immediately fixated to the peripherally seen, and the final organization of the whole into larger units, would be merely shoved back by such an explanation from word units to letter units. The real succession in the perceptive process would remain undiscovered and unsought. The process by which new perceptual units come to take the place of old ones in normal life shows how definite is the central complication on which the unit depends. It takes long training to 'see' the minutia of technique. It takes long training to dissociate angular perceptual units and 'see' them as lines instead of as areas, but it can be done. And while the process may be associated with definite mannerisms of visual fixation, it is a peculiar fact that once the unit is established by a sufficient course of training, it may remain uneffected by chance variations in the clearness of details or in the exact position of the point of regard.

What the central conditions are which determine, in any given instance, the perceptual units is at present a matter of conjecture and interpretation rather than of experimental demonstration. Judd holds that the organization of sensory data into units as well as its organization in general is primarily a motor fact. In so far as my own process of reading is concerned I believe his contention is justified by the facts, but the immediate motor organization in my case is articulatory rather than visual. Whatever the elements are and however they are conditioned as units, our present interest centers in the process by which the new, relatively indefinite, peripheral material enters the complex perceptual process, while the fixated unit is clearing up and receiving its last sensory impulse to clear perception.

The nature of the data involved in the pre-fixational vision may be determined by direct observation. In the first place it

is not individual letters. From any given fixation point individual letters cease to be clearly seen at a distance much less than we found reason for asserting the pre-fixational beginning of perception. The only thing that is distinguishable half or two thirds across the page is a vague outline of the word, a vague word form, if, in spite of recent criticism, we may still continue to use the term without the formality of a justification. In many cases, it would be altogether impossible to recognize the word from this vague outline. In all cases its perception is doubtless delayed beyond the normal perception time for objects at the macula. The question is: What can that vague outline do towards the initiation of the subsequent clear perception? I conjecture that as a stimulus its influence is general rather than specific. I conjecture that the pre-fixational stimulation is a general stimulation of a considerable group of verbal residua. And prolonged observation of the peripheral field supports the hypothesis, since from the shadowy outlines of the peripherally seen words a succession of words may tentatively arise, more or less similar in general appearance, which we may test out by comparison with the peripherally seen word until we find one that fits. In the normal reading process there is no time for any of these suggestions to be tested out; they doubtless never pass the stage of mildly aroused residua, belonging to a general group. As in subsequent fixation the peripherally seen word comes to the area of clear vision I conjecture that the inhibitory function of clear perception becomes more prominent, shutting out of the competition all of the residua aroused by the more general peripheral stimulation except those further stimulated by the new, more definite details. Whereas there is absolutely no evidence in normal reading for a successive constructive synthesis of words out of their letters, I believe there is evidence for a more or less gradual selective discrimination in favor of the more fitting components of a more or less general stimulation. The processes of stimulation and inhibition that characterize all neural action are doubtless no more exactly simultaneously balanced in visual perception than in ocular motion.[1] And there is certainly some evidence for the as-

[1] Compare *Yale Psychological Studies*, Vol. 1, No. 2, p. 386, fol.

sumption that the progress of the development of an individual nervous excitation follows the direction of the development of nervous impulses in general, *i. e.,* from the general to the specific. Aside from the purely visual pre-fixational stimulation of the verbal residua, there are unquestionably other pre-fixational factors entering into the pre-perceptive arousal of verbal residua. Most conspicuous of these other factors is the development of the meaning consciousness. At any given point in the reading of non-technical, familiar discourse there are relatively few words that would serve as fit continuations of the matter already read. This development of the sense may be our only clue to missing words, to misprints, and to partially blurred words. It may enable us to read aloud words on the next page before it is turned, and may even on occasion lead to the illusion that the word has been printed twice, at the end of one page and the beginning of the next. Similarly familiar phrases may be pre-perceptually completed; and undoubtedly rhyme acts in a similar manner. Any of these factors, and there are doubtless others almost as important, may operate chiefly as a general stimulus or chiefly as an inhibitory selective influence on a general stimulation already operative. But whatever the origin of the stimuli, whether objective or subjective, whether visual or interpretative, it is evident that some are more general than others; while in some the inhibitory restrictive elements are more prominent. It is evident, furthermore, that the pre-fixational, peripheral visual stimuli belong to the former class, rather than to the latter.

This theory of the value of the peripheral processes serves in a measure to explain the length of the reaction time of the eye. Contrary to our psychological tradition, it is not essential to the best development of the visual percept to transfer a peripheral stimulus with the utmost celerity to the fovea. The peripheral vision itself has a real part to play in the total process and to inhibit the peripheral stimulation before it had visually cleared up would be to lose that value. Doubtless this factor interacts with the necessity for some elaboration of the motor impulse that shall bring the new object of interest to the area of clear vision. Unquestionably the peripheral

vision is sometimes satisfactory enough in itself without demanding clearer vision. Such is undoubtedly the case in reading for zones bordering on the macula.

Just as the new visual stimulus, breaking into the pre-perceptual field must last long enough for an adequate clearing up, so the pre-perceptual material breaking into the mental complex may pass gradually, by a selective inhibitory process, into an adequately cleared-up consciousness of the object itself. On the other hand there is abundant evidence that it may operate to modify the general conscious complex into which it enters without ever consciously clearing up at all. Jastrow's experimental evidence of the influence of invisible lines is to the point. Moreover, the peripheral pre-fixational factors, under ordinary circumstances, never clear up in consciousness as independent percepts. The disturbing lines in an illusion are never immediately recognized as such without some sort of experimental analysis. The peripheral setting of a visual object modifies our attitude towards it without necessarily clearing up in consciousness. Furthermore, there is some evidence that even within the object of regard, foveal or near foveal elements may operate to modify the total resulting consciousness without any proper clearing up in consciousness. An illustration of the fact occurs in the consciousness that an exposed word was incorrectly spelled, without a corresponding specific consciousness of what or where the error was. Doubtless the individual letters operate in a similar way in normal reading.

Such non-independent processes, whether peripheral or central, have been variously named subconscious, subliminal, or fringe phenomena. They are all included under the purely descriptive, generic term, uncleared processes. This includes the faint subliminal stirrings, as well as the otherwise adequate stimulation which encounters inhibitory central resistance in the form of preoccupied attention. It includes, furthermore, those stimulated memory residua that obviously modify our appreciation of present conditions without appearing in the form of memory images. Among these latter clearly belong the individual word meanings and their immediate con-

tribution to the total meaning consciousness of what we read. The concept of the uncleared processes is as old as Aristotle and recurs in ever new forms in the history of psychology. That presentations have a period of latent incubation, a period of clearing up, and a period of elaboration, receiving elements from the unevenly disappearing conscious field which it displaces, and leaving more or less persistent residua to complicate the clearing up of succeeding fields; and finally that these uncleared factors coexist in a bewildering complexity during every moment of consciousness: all this is no new doctrine. My task has not been to demonstrate the obvious, but to attempt to trace some of the simpler lines of complication in the process of visual perception and disentangle some of the simpler relationships in the process of reading. Here at least the process is capable of some analysis and we can demonstrate how the total consciousness is modified, partly by the vague peripheral vision, more sharply by the clearer vision of the macula, less conspicuously perhaps but just as really by the continuance in peripheral vision of a considerable succession of previous fixation objects. In a real sense, phrase, sentence, and paragraph, episode and plot form a 'dynamic background,' or setting, for each new word complex as it clears up. And each new word complex in passing adds its modicum to the more slowly developing consciousness of meaning which in turn becomes part of the setting for still later elements. An adequate experimental analysis of these apperceptive complications has yet to be made. Consequently the relative position of the visual fixation pause in the total complex can at present be estimated only roughly as somewhere in the middle of the process, between the pre-fixational vision and the utterance, or appreciation.

§ 3. *The Determination of a New Point of Regard.*

Among the concurrent perceptual processes, the one most intimately connected with our previous investigation is the reaction by which the new peripheral object of interest is brought to the area of clear vision. The comparative length of the ocular reaction time and the brevity of some of the fixations

forced us to admit the possibility of a pre-fixational origin of the ocular reactions in reading; that is, that the impulse to a given eye movement may have had its origin at some point in the reading process more remote than the immediately preceding fixation pause. The possibility of such a predetermination of the eye movements was already demonstrated in the case of those rapid return eye movements in which the return sweep followed the outward sweep so quickly that there was no adequate clearing-up interval, cf. page 48, and consequently no possible occasion for an adequate visual stimulus for the return movement. Furthermore, all competent observers agree that in reading the eyes follow a more or less unstable rhythm, with characteristic individual tendencies. But while the number of fixations show what may pass for an habitual rhythm there is less uniformity in the extent of the eye movements between fixations.

It is a familiar dogma of psychological optics that the point of regard coincides or tends to coincide with a point of interest, but neither in the assumption of a necessary *point* of interest, nor in the assumption that the latter even if it existed would ever exactly coincide with the point of regard does the dogma correspond with the facts. On the contrary it has been shown that if the object of regard happened to be a point it could never coincide with the point of regard except by accident, on account of the persistent physiological disturbances of fixation. We have seen further that the object of interest in normal reading is practically never a point but an area, whose dimensions are determined not merely by visual expediency, but largely also by complex central conditions. While it is true in general that the point of regard will tend to lie within the object of interest, even this generalization must be limited both temporally and spatially. First the object of interest regularly becomes such because of its relation to a previous object of interest and usually it becomes such while it is still seen only peripherally. But the wide variation in altogether satisfactory fixations makes it possible that in viewing a succession of relatively small objects of interest, the point of regard should not fall on any one of them. Cases to the point are the viewing of letters, words, and groups of words, see page 22.

In all this it appears again that the precise position of the point of regard with respect to the object of interest is a matter of relatively small importance. For example a short word, like the word *object,* is visually as clear when I fixate the end as when I fixate the beginning. It is as easily apprehended from one point as from the other. Moreover, the speech reactions show no univocal variation in favor of any one fixation. It would be strange therefore, especially in view of the physiological variables which are involved, if the point of regard should fall on the same part of the word in successive fixations, and there is abundant evidence that it does not.[1] Further evidence as to the insignificance of the exact position of the point of regard in reading is seen in the entire absence of the minute corrective movements in rapid reading that always occur in any attempt to fixate a point accurately.

The previous question what determines the precise position of the point of regard at any fixation pause must be answered negatively—no one factor determines it either psychological or physiological. Positively, on the other hand, one may say it is determined (1) by the unsatisfactory pre-fixational perception of a peripheral object of interest;[2] (2) by the central habitual predisposition; (3) by gross accidental variations due to the chance arrangement of numberless physiological factors. Of these three general conditions obviously the third merely prescribes the limits within which the first two are operative. At the same time it gives some indication of the questionable value of the painfully exact determination of the position of the point of regard. However, we must admit that, within certain considerable limits of error, the successive positions of the point of regard give some indication of the extent and character of the corresponding objects of interest in the complex perceptual process. The way in which the fixation works up into the angles of a Mueller-Lyer illusion, when the illusion is most strongly seen, seems to indicate not so much the motor organization of the visual processes as rather the obvious fact that, in spite of intent and introspective assurance, the real object whose partial pre-fixational perception occasions the eye

[1] Dearborn, *The Psychology of Reading,* chapter XI.

[2] See Dearborn, *op. cit.,* p. 91.

movement is something other than the exact point of intersection of the lines. It seems to me it would be absurd to hold that the point of regard within the angle necessarily coincided with the imperfectly seen peripheral object of interest. In this case doubtless as in reading the true object is not a point but an area. While the actual point at which the point of regard rests is quite accidental, save in its general relation to the area of interest.

Similarly in reading, while the exact position of the fixation point is usually without significance as an indication of a *point* of interest, the length and number of the eye movements must be a reasonably safe guide as to the perceptual units and the completeness with which pre-fixational data satisfy the requirements for clear vision. For instance, the way in which familiar phrases are satisfactorily apprehended from a single fixation seems to indicate that in some cases the phrase itself may become the perceptive unit.[1]

[1] Dearborn, *op. cit.*, p. 86.

Chapter IV. The Organization of Retinal Elements.

§ 1. *The Function of Extra-foveal Vision in Space Perception.*

Since no object, however minute it may be, is confined during fixation to the fovea alone, and since most objects of regard actually clear up as a result of stimulation which is largely outside the macula, while even the peripheral retinal areas regularly add their part to the complication of the central processes; the function of extra-foveal vision refuses to be regarded as merely preparatory and secondary. On the contrary in at least one typical and rather exacting visual process, *i. e.,* that involved in reading, we have seen that extra-foveal vision is an integral and invaluable factor in the total product of each fixation; without which each successive reading pause would add only a relatively insignificant detail to a relatively unorganized mass of experiences. The very length of the ocular reaction bore testimony to our general contention. Even when the object of regard happens to lie entirely within the fovea, foveal vision is never isolated and distinct from the extra foveal. Ordinary observation gives no adequate clue to the position of the point of regard. Every object of regard is seen as a part of a simultaneous field. And the field, real or imaginary, is a significant factor in the perception and interpretation of every discreet object of regard. Moreover, the field is not a mere mosaic of previous foveal stimuli; for each foveal stimulation is only a part of a whole in which a distinction between the foveal and extra-foveal never appears. Physiologically and psychologically, the only functional distinction between different zones of the retina is determined anew in each act of vision, according to the adequacy of the resulting vision.

Doubtless the most universal as well as the most conspicuous relationship between the various retinal areas is their spatial organization. With one or two notable exceptions, it has been the custom in psychological discussion to view the

spatial organization of the eccentric retina from the standpoint of the relatively more important and refined organization of the fovea. Extra-foveal vision has been consistently described and evaluated by its relation to foveal vision. Certainly in the case of a single eccentric object of interest there is obvious justification for the traditional precedure. But notwithstanding the preponderance of scientific judgment, the new facts at our disposal with relation to the fixation movements and the increased significance of peripheral vision, persistently raise the question whether the traditional view is not one-sided. Are the spatial relations of the total visual field determined by its relation to the fovea, or is the object of regard not rather determined in its spatial relations by its apparent position in the total visual field? Personally it seems to me that were our judgment not influenced by the deference due to a long line of theoretical considerations we would be inclined to adopt the less popular alternative. Certainly that would correpond more closely with the facts of experience as they are amenable to direct observation. For example, the horizon is not commonly apprehended by referring to it some minute foveal object of regard, the latter is oriented rather with respect to the horizon.

In view of the facts of immediate experience, in view of the incontestible facts of ocular movement as disclosed by Stratton, and by Judd and his assistants; and finally, in view of the lack of a definite intra-foveal fixation point; the question forces itself more and more strongly upon us whether the more general traditional view of the spatial organization of the retina does not need thoroughgoing revision. The task is a serious one and it obviously does not fall within the scope of a discussion of visual fixation. But, on the other hand, the data which force the reconsideration suggest a line of acceptable reconstruction, centering in a new theory of the development of retinal local signs.

§ 2. *Retinal Local Signs.*

A satisfactory local sign must serve two functions. It must serve to differentiate some particular retinal element from

every other, and it must serve as a bond between the differentiated element and the other interrelated parts of the retina. The motor impulse, by which a peripheral object of regard would be brought to a supposed fixation point at the center of the retina, would seem to be admirably adapted to the purpose. In the amount of the necessary innervation, and in the specific combination of the eye muscles involved, each retinal point would not only be differentiated from every other point, but through the interaction of the different muscles in different degrees for neighboring points, all the retinal elements would be united in a relatively simple closed system, without duplication, and without hiatus. The theory was originally proposed purely on theoretical considerations and it maintains its position at the present time on purely theoretical considerations. As far as I am aware there is not one particle of unambiguous experimental evidence that can be adduced to its support. The closest approximations to such evidence are the familiar facts that we do move the eyes to bring objects of interest to an area of clear vision; and that there is a certain demonstrable kinæsthetic consciousness of ocular position and of ocular movement. The grossness of the errors of the latter data are evidenced by the mass of entirely unsuspected facts respecting the eye movements, that have been brought to light by better means of direct observation, and by objective registration; all beginning, I believe, with a demonstration of the fixation pauses in reading. The relatively large eye movements during the consequent fixation of minute objects of interest was first brought to light by the refined technique of recording apparatus.

In addition to these rather dubious supports is the alleged evidence that the perception of motion in the third dimension follows laws that are most easily interpreted as a perception of convergence movements of the eyes. But when it is considered that there could be no impulse to convergence except as a response to some adequate stimulus which indicated the need of convergence; and that this antecedent stimulus would furnish the most immediate and direct evidence of movement in a third dimension; and when it is further considered that

all convergence movements permit clear vision during the movements of convergence, and that consequently they are all accompanied by corresponding retinal impressions, it becomes obvious that convergence movements of the eyes have no peculiar or unusual fitness for mediating the perception of depth. And finally, when it is considered that in simple convergence movements without either of the above factors, as when a binocular after-image, sufficiently strong to persist some time after closing the eyes, shows no apparent motion in the third dimension, notwithstanding the general tendency of the eyes to relax all convergence when at rest, and even in spite of actual voluntary convergence, the alleged evidence from the convergence movements becomes not only doubtful but improbable.

While there is no direct and unequivocal evidence capable of demonstrating the traditional theory, there is a growing body of evidence of its inadequacy. Probably the most conspicuous result of all the recent exact data with respect to the eye movements is the evidence they force upon us of the utter insufficiency of the ocular kinæsthetic data. Neither the velocity nor the extent of our eye movements is directly known to us. We have no immediate consciousness of their direction. Usually we are unaware of their existence. All this evidence comes as a shock to the self-complacence with which eye movements and eye strains have been exploited by the introspectionists. But along with the necessary changes in our traditions, which must follow the knowledge that our eye movements are not adequately represented in consciousness, may come the correlate that, so far at least as the theory of motor local signs are concerned, direct consciousness of eye movement is altogether unnecessary. It is not necessary that the local sign should be at once both a psychological and an identical physiological fact. Indeed when we stop to think of it the identity of the two series in any element of our experience would be a matter of far-reaching significance, which we would not take as calmly as we acquiesce in the local sign theory. The main functions of a local sign would be subserved by the physiological facts of ocular adjustment, quite irrespective of their direct representation in consciousness. To ask for the latter in the case

of retinal organization would be to demand more than we have to be content with in many other forms of mental organization. At least we must agree that the lack of immediate consciousness is no valid argument against the operation of the motor impulses in a differential organization of the retinal elements. But the theory loses its supposed introspective support; and the original form of its statement must be modified to eliminate the word *sensations* of movement. Adequate sensations of eye movement simply do not exist. Whatever the motor factor in the spatial organization of the retina may be it comes to consciousness not as sensations of movement but as spatial relations.

The question whether the oculo-motor impulses, as we now know them through the medium of exact registration, are adequate to account for the spatial organization of the retinal elements seems to me to represent the real crux of the problem. And there is a considerable body of cumulative evidence, without a single negative element, that they are not. The first evidence in this connection was published by Dearborn.[1] He found that the average error in the motor impulse by which the fixation of a peripheral object was brought about was approximately ten times as large as the local discrimination, when both were measured at a point on the retina 40° from the point of regard. McAllister found that there was a disproportionately greater error in the movements to fixation when the object is only 10° removed from the point of regard.

But in addition to this we must also take into consideration the changes that have taken place in our conception of the fixation process itself. Since, as we have seen, there is no true anatomical and functional central point of the retina, but only a more or less constant area of clearer vision, the traditional theory is robbed of the hypothetical point around which the motor organization could take place. Neither in the definiteness of motor impulses, nor in the definiteness of a fixation center are the necessary conditions present to make an exclusively motor organization of the retinal elements a tenable hypothesis.

[1] Psychological Review, XI., 297–307.

Furthermore, as Lipps has pointed out with such vigor, the general conditions of the operation of the motor impulses in developing a differentiated organization of the retina, presuppose a degree of mental development and retinal differentiation which, if it existed at the beginning, would render the later development a useless and a gratuitous complication. Namely, there is only one motive for bringing a peripheral stimulation to the central area of the retina and that is lack of clear vision. Conversely there is only one final indication of the fact that the object of interest has reached the central retinal area and that is the relative clearness of the vision. But clearness and a lack of clearness are not primitive attributes of retinal sensations; they involve highly complex conditions of retinal organization and of perceptual elaboration. The only possible meaning to the clearness of a retinal impression involves the adequate discrimination of its parts, but until we have an organized retina, with a complete system of local signs, there will be no psychological parts of a retinal impression. Indeed not until there is considerable development of the total perceptual process can we speak of a unitary object at all. If it be objected that the tendency to bring the peripheral stimulus to the macula is an impulse independent of the matter of clearness then we must insist that such a hypothetical tendency independent of the matter of clearness must have died out in mature life, since it is conspicuously absent in the normal functioning of the eyes as illustrated by the process of reading. While if it really ever did exist it would indicate a pretty complete differentiation of the retinal elements antecedent to all movement.

A further difficulty in the traditional motor theory of retinal organization is the implication of the high degree of abstraction which it involves. Only rarely in primitive conditions would a visual field be composed of a single punctiform peripheral stimulus. As a matter of fact the typical retinal stimulus is a highly complex mosaic, all of which moves across the retina in consequence of the same eye movement that moves one particular peripheral point to the center of the fovea. But the point that actually comes to the center of the fovea cannot

be distinguished as a peripheral point without the operation of a considerable degree of abstraction from the surrounding stimuli, which in turn would presuppose a relatively high degree of retinal differentiation. As a matter of fact, except on the presupposition of an already organized retina, every motor impulse would be associated not with one retinal element but with the total changes throughout the whole retinal field, while the movement of the point previously at the fovea would tend to be rather more prominent than the obscure peripheral stimulus. The association of the movement with one single change is doubtful under the most highly developed conditions, but even if it obtained it would presuppose a differentiated apperceptive process which we have no grounds to assume in primitive conditions.

Finally, there is no ground either in adult life or in the observable eye movements of infants to assume that there is any motive to bring foveal stimulation to the exact center of the fovea. Indeed if such a motive had once obtained it must have perished before adult life. Records of adult eye movements indicate neither the motive nor the fact. As a consequence the motor theory of retinal organization even if it satisfied all the other difficulties which we have discovered, would break down absolutely at the very point where the organization of the retina is the most refined, namely, at the fovea itself. But any supplementary theory which would account for the foveal organization must also account for much more. I believe the conditions of such a theory of the organization of the retina are present in the general conditions of retinal stimulation, together with the involuntary fixation movements which we were able to demonstrate in the first chapter.

It is only fair to my general contention to point out at this time that the evidence against the adequacy of the motor local signs of the retina is not in any way immediately referable to the tactual local signs. Indeed I believe that we should carefully distinguish between the general character of the eye movements and the character of the other reactive somatic movements. The eye movements, in so far as they are integral parts of the visual process, are never the consummation of the process into which they enter. They are subsidiary to the

retinal and dioptric processes which they condition, and their chief function is to provide for a more adequate retinal stimulation. They are never intended as the final response of the individual to adequate knowledge of his environment; they never constitute his real reaction upon his environment. In this sense they differentiate themselves from conduct proper as belonging to a preparatory stage. They constitute action for the sake of knowledge rather than action upon knowledge. Doubtless the fact that they never constitute the real end of action has much to do with the relative inadequacy of their representation in consciousness. They contrast sharply with the movements of the head which in the lower animals almost entirely supplant the long eye movements that are observed in man. When a dog moves his head towards an object of interest his action is not merely preliminary to the clearer perception of that object, it is a part of his general adaptation to a situation. Whatever the new object of interest may turn out to be, he has executed a movement of strategic value and is in a better position to act upon any new intelligence which the movement brings him, whether it is the occasion for defence or for offence. This peculiarity of the function of the eye movements doubtless corresponds closely with their place in the conscious life. They are only partially voluntary, and are altogether withdrawn from voluntary control. Once they have begun, they can neither be hastened nor retarded, and we are impotent to change their direction. Finally, as we have seen, they yield only few, indefinite, and unreliable sensations of movement.

On the other hand we must not overlook the fact that the eye movements are closely related with the movements of the head and body through the general functional correspondence between the two, and especially through the coördinate compensatory eye movements. While the sensations of eye movement are conspicuously lacking, the motor mechanism for coördinating all forms of movement in response to particular needs are conspicuously adequate. They are much more refined than the coördination between peripheral retinal elements and the impulse that brings their stimulus to the fovea. So that, whatever function the eye movements may have in the

development of our visual space, the mechanism is already obvious that would serve to unite visual and tactual systems into an organic whole.

§ 3. *A Genetic Organization of the Retinal Elements.*

Notwithstanding the inadequacy of the sensations of movement and the motor impulses to account for the differential organization of the retinal elements, the fact remains that the latter are most delicately organized. This is true not alone in the so-called higher vertebrates, but also in the lower vertebrate eyes, where the mobility is much below that of the human eye. And there is no evidence that the organization of the retinal elements in the lower form of the vertebrate eye is any less complete than it is in man. Whatever the origin of the retinal organization is, then, it must be some primitive process, not too dependent on the movements of the eye, and not necessarily dependent on a high intellectual development. I believe there are factors in the most elementary retino-central processes, which must tend to give the retinal elements a physiological organization, corresponding point for point with that organization of the retinal impressions which we know in conscious life as spatial.

If, for the sake of logical completeness, we may postulate an absolutely undeveloped retina, a sort of *tabula rasa* affair, which, in its anatomical relations with the great nervous centers, is otherwise perfect, save in the matter of the mutual relationship and differential organization of its parts, it is obvious that the first stimulation by a complex visual field will produce a complex mosaic of retinal stimulation, corresponding with the general conditions of the dioptric apparatus and the anatomical arrangement of the retinal elements. But such a complex mosaic of stimulations would have neither unity nor a differentiation of parts; it would be completely unorganized. The differences of stimulation, however, must bring about certain changes in the different anatomical elements of our functionally undifferentiated retina. Any group of retinal elements *n* which is covered by the same patch of stimulus; merely by virtue of the similarity of the stimulation of its various

parts, thereby differentiate themselves in their life history from all parts of the retina which are otherwise stimulated. Normal complexity of the visual field will produce an indefinite number of such groups of elements, which are already differentiated in their physiological relationships from each other and from all other parts of the retina. But the eye is in constant motion. Entirely apart from all visual motives, we have seen how the purely physiological facts of pulse, respiration, and defective muscular balance must unite, even in a totally undifferentiated state of the retina, to produce a more or less continuous eye movement. Consequently, even if we admit no change in the mosaic of light and color, which stimulates the retina, the groups of retinal elements, which each part of the mosaic functionately associates, will suffer constant rearrangement. Each element in the first *n* group becomes in turn temporarily associated, because of the momentary identity in their life histories, with various other elements on all sides, which latter are in turn related to others. Thus, while the differentiation involved in the variation of the neurone life history constitutes a peculiar property for each retinal element, it also unites it, simultaneously or successively, with all other retinal neurones; immediately with its neighbors and mediately with the entire retina.

The process may be diagrammatically represented if for simplicity's sake we reduce it to the integration of a simplified

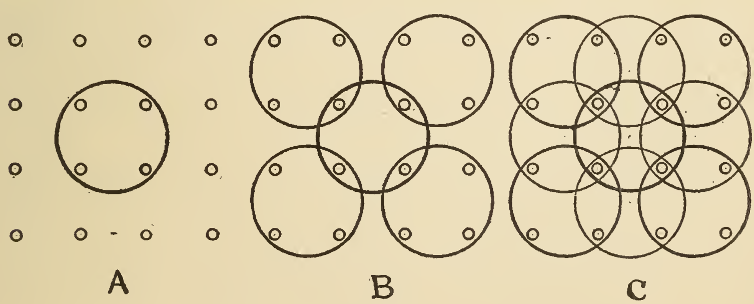

Fig. 4.

retina consisting of sixteen elements, and if we reduce the complication of the visual mosaic at the same time to a single patch of light, covering, at the first moment of stimulation, little

more than the central group of four elements, as in Fig. 4, *A*. Already, as the result of the first moment of stimulation, the central group will have become differentially associated, by reason of that peculiar difference in the life histories of the neurones which the stimulation effects. The result of involuntary fixation movements in all directions will yield a successive complication of the original groupings, corresponding to Fig. 4, *B* and *C*. A still better illustration of the differential organization would be made by a similar group of sixteen pegs, in which the differential organization might be represented by rings of twine.

The frequent repetition of these successive groupings must give them permanency. And the natural variations in the form of the stimulation must give the organization of the groups a refinement which is lacking in our first stimulation. Moreover successive stimulation must perpetually tend to unite into closer and closer functional relationship those elements that are more often similarly stimulated. And all the relationships so established are preëminently adapted to coördinate with the relationships of the tactual sense in the complex organization of our sensory experience which we have given in immediate consciousness as space. Whether any individual eye is ever forced to build up such an organization *de novo* is doubtful. The organization as we have described it is admirably fitted to be perpetuated more or less completely by an inherited anatomical organization. And the evidence is that long before there is delicate muscular control of the eyes or of any other part of the body the organization of the retinal elements is relatively complete.

In addition to the differentiation of the retinal elements, effected by objective differences of stimulation, doubtless the qualitative differences on which Wundt lays emphasis coöperate to the same end. But in view of the differential organization effected by group stimulation the burden falling on the original qualitative differences in the retinal elements is more nearly proportionate to their known differences. They would serve rather to differentiate and to associate relatively large concentric areas.

Moreover, pursuant to our general theory, it would be the physiological rather than the psychological qualitative differences that would come into consideration. The psychological aspects of the retinal organization are given in the spatial relations of the various parts of the field of view; and it is doubtful if the psychologically qualitative differences ever enter consciousness as local signs, or in any other capacity, until they are brought to light by the analysis of the psychologist. Common experience does not know the local differences of color perception, nor the hiatus of the blind spot; and it is difficult to understand how these qualitative differences could become operative under the most favorable circumstances except after a prolonged discriminative experience. Finally we are seeking the basis of that physiological organization of which space relations are the psychical correlate. The attempt to discover an identical fact in the two series seems to us as hopeless as it is useless. Success would not bridge the gap; it would only emphasize the differentiating elements and complicate the problem.

While the group differential organization of the retinal elements has been progressing certain gross differences must have been developing in the various parts of the retina. Since the differential organization depends on the definiteness and the complexity of the mosaic of stimuli, that part of the retina, on which the images formed by the dioptric apparatus are sharpest and consequently most numerous, will have a definite temporal advantage in its development over peripheral parts of the retina, and this temporal advantage in development will account for the origin of that more intimate connection with our cleared-up visual consciousness which later experience has abundant motives for maintaining. In this point again our genetic organization theory seems to me to have an advantage over the traditional local sign theory; since, according to the latter, unless we admit a certain amount of local discrimination at the beginning under the name of clearness, we must fall back on some otherwise unexplained inherited tendency to account for the tendency to move the peripheral image to the mythical retinal center.

Finally a further complication has been progressing. Along with the group differential organization of the retina, the whole system has been coördinated with a certain relatively late and relatively gross organization of the motor impulses. With every accidental eye movement there must have been established a bond of association between the changes of position in the group stimulations of the visual field and the concurrent motor readjustments which occasioned them. This association must tend to standardize the group organization at various parts of the retina; and, as a matter of fact, that is exactly the peculiarity which is most striking in the organization of the retina. Notwithstanding some significant variations like the differences in the upper and lower visual fields, that appear to me to resemble more nearly the complication of later experience rather than the result of the primary organization of the retinal elements, there is a remarkable unanimity in the spatial values of similar stimuli on different parts of the retina. This relative unanimity does not follow in any way, directly or indirectly, the laws of muscular strain. In order to be explained by the motor local sign theory the latter must invoke some complex standardizing product of experience, or else force the motor organization to subserve not only the local sign of a given retinal element but also the general change in the position of the entire visual field.

But the same associated motor organization that standardizes the different parts of the retina through its simultaneous concurrence with the changes at all points alike, may also serve as a connecting bond between the organization of visual and the organization of tactual elements. This again is directly congruent with the facts as we know them in the coördinate compensatory movements; and in the ability to roughly adjust the eyes with respect to any arbitrary displacement of the head, totally independent of any purely visual motives. It is congruent furthermore with the feel of the eyes for definite positions within the socket which is consciously associated, not with any particular point of the visual field as its local sign, but with the relative position of the area of clear vision and the whole visual field with respect to its visible boundaries and the

position of the head. In this respect our theory has a further advantage over the traditional theory, in that it leaves the motor organization unburdened by work which it could never adequately perform, and consequently free for the work which it very obviously does perform.

Finally the application of the traditional theory to the perception of a complex visual field such as obtains in reading imposes an uncommon tax on one's credulity. If it might be assumed for the moment that the conditions of absolute fixation which the traditional theory assumes were really a fact, then the fixation of a single complex letter like an *a* would involve the arousal of retinal signs in the form of differentiated motor impulses in every direction from the point of regard. But the motor impulses to move the eyes in every direction would produce a more or less balanced tension of all the eye muscles which must, in the nature of muscular innervation, not only be fatiguing, but it must produce marked irregularities of the muscular balance and consequently of fixation itself. When the visual field is complicated by the addition of other letters sufficient to make a word, each point in the new stimulus will be differentiated only by the motor local signs and the new groups of stimuli will increase the delicately modulated pulls on all sides, which must be further increased in complexity as well as in tension by the total stimulation of a simultaneously exposed page. A simultaneous presentation of movement in all directions is disconcerting enough. But the simultaneous presentation of an infinite gradation of movement in all directions might well serve as an illustration of the utterly self-contradictory. While a corresponding increase of the tension of the eye muscles by a complication of the visual field is not a fact. The substitution of kinæsthetic residua for actual muscular innervation relieves the situation only with respect to the actual muscular tension. The self-contradictory absurdity of the attempted presentation still confronts one.

In contrast to this impossible conception appears the differential organization of the retinal elements in groups. The complex form of any particular grouping is a matter of entire indifference in the associated life histories of neighboring ele-

ments. And the complication of the visual field as it is found in reading instead of leading to a self-contradictory absurdity, must be regarded as only a slight increase in the complexity of the conditions that originally produced the physiologically differentiated retinal elements.

Appendix. The Technique of Recording the Eye Movements.

Since the final appeal with respect to the credibility of an alleged fact of eye movement, fixation, or muscular activity must be made to an objective record the technique of recording the eye movements becoms *ex officio* a necessary even if a subordinate part of our discussion.

The first reliable device for recording the eye movements was the mechanical registration apparatus of Delabarre.[1] Published simultaneously with the apparatus of Delabarre appeared the ingenious modification of the apparatus to meet the exigencies of recording the movements of the eyes in reading by E. B. Huey. In spite of its intrinsic worth it is improbable that mechanical registration by direct attachment to the eye will ever come into general use; partly because of the exactions of the technique, together with the extreme penalties of carelessness; and partly on account of the unaccustomed load which the eye muscles are compelled to carry. But Delabarre's plaster cup attachment has the honor of being the first really fruitful device to meet the new scientific requirements. Personally I cannot but regard it as a scientific misfortune that the elaborate material in his possession has not found fuller publication.

It was in consideration of the theoretical difficulties which appeared to be involved in registration by mechanical attachment, and before I was aware of Delabarre's success in overcoming them, that I worked out a plan for photographing the horizontal eye movements. Then and now a pencil of light appealed to me as the only available registering medium which had neither momentum nor inertia, which was absolutely safe and universally available. But the development of a satisfactory technique was a long process of experimental elimination and modification. The most serious of the early difficulties

[1] E. B. Delabarre, 'A Method of Recording Eye Movements,' *Am. Jr.*, Vol. IX., pp. 572–574.

concerned the lack of sharply defined optical contrasts on the eyeball. By a series of accidents and theoretical considerations in conjunction with the patient help of one of my pupils, T. S. Cline, Wesleyan, 1902, I came finally to use the reflection of light from the cornea. Unquestionably the most brilliant advance in connection with the photographic registration of the eye movements was made by Judd, when as the result of painstaking experiments he succeeded in finding an innocuous speck of brilliant white that could be placed on the cornea and photographed with a kinetoscope camera. Undoubtedly Judd's apparatus in its new form, giving practically a continuous exposure, is capable of getting accurate results under a greater variety of conditions than any other objective method now in use. I cannot but regard it, however, as a misfortune that with the general recognition of the high value of Judd's apparatus and the results he has achieved with it, there has developed a widespread but I believe an uncritical distrust of the method for which I feel a certain amount of personal responsibility and which for some kinds of work at least has I believe no superior. The criticism to which the method has been subjected, together with the use that others have made of it, that I am now making and plan to make, forces me to a formal discussion of its theory, the limits of its reliability, and its present technique.

After all the fundamental question with respect to any technique is whether it is serviceable. That I take to mean whether with intelligent use and adequate control, it is capable of extending our scientific knowledge concerning matters of importance; secondly, whether its incidental errors are capable of elimination or determination; and finally, whether, considering the results obtained, it is reasonably economical of time and laboratory equipment. It is by these tests that any method of registering the eye movements must stand or fall. No technique is responsible for carelessness or uncritical misuse.

When first published, my method was primarily adapted to measure the angle velocity of the eye movement. And until the present discussion, my own immediate use of the method

has been based rather on its reliability in measuring the temporal characteristics of eye movements than their exact spatial relations. But its ready adaptation, under proper control, to measuring the extent of eye movements as well as their duration led to an extension of its use. Doubtless some mistakes were made. Doubtless too the empirical control was not always adequate but such difficulties have confronted most methods with which I am acquainted. They would be poor excuse for announcing the obsequies of a technique or for too sweeping generalizations with respect to the limitations of its application, at least not until it has been given a fair experimental trial for its new work.

§ 1. *The Theory of the Movements of the Corneal Reflection.*

The theory of the method, briefly stated, is that the virtual images from an eccentrically mounted convex spherical mirror appear to move in the direction of the latter's rotation when its axis lies behind its center of curvature. Now within a very small error the surface of the normal healthy cornea is a convex spherical surface, as exact as the general visual process which it conditions. If the radius of the curvature of the cornea were infinitesimal, the apparent movement of the corneal reflection would equal the sine of the arc of movement measured on a great circle of the eyeball. If, on the other hand, the radius of the cornea were equal to the radius of the eyeball, and the latter rotated on its center of curvature, the corneal reflection would appear to remain stationary. As a matter of fact neither of the above suppositions is true; and the apparent movement of the reflection actually lies somewhere between zero and the sine of the angular movement of the eyeball. More exactly, since the average radius of curvature of the center of the cornea is 7.7 mm., and the distance from its apex to the center of rotation of the eye averages 13.5 mm., the apparent movement of a distant object reflected from near the center of the cornea will be slightly less than one half the actual displacement of the apex of the cornea but always in the same direction. More accurately, under the above conditions, the movement of the reflection will be $(13.5-7.7)/13.5 =$

(5.8)/13.5 of the actual movement, as may be proven by the following demonstration.

Let Fig. 5 represent the horizontal cross-section of a schematic eye situated before a camera and fixating successively points lying along the lines of regard *CL* and *CR*. Let the apparent source of light be at the center of the photographic lense. Then the only beam of light that will be reflected back to the lense from the cornea when the eye is in the position *CL*

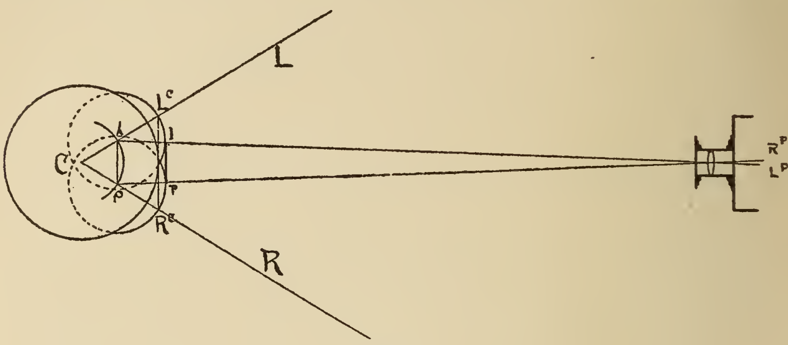

FIG. 5.

will be that whose line of reflection from the cornea is identical with its line of incidence, namely, that beam which if continued would pass through the center of curvature of the cornea. Consequently the apparent image of a punctiform source of illumination will be approximately at λ.

If the line of regard moves through the angle *LCR* the apparent position of the virtual image will change to ρ. Since as the cornea moves with the eye, the beam of light which will be reflected back to the lense from the new position of the cornea will strike the latter at *r*. But by the properties of similar triangles

$$\lambda\rho : L^cR^c :: C\lambda : CL^c. \qquad \text{Q.E.D.}$$

Somewhat unfortunately for the purposes of photographic registration the radius of curvature of the cornea increases slightly from its apex to its periphery, where it is highly irregular. It follows: (1) That no use of the corneal reflection is permissible which involves the use of the extreme peripheral and irregular portions of the cornea; (2) that all records

produced by eccentric portions of the cornea will be foreshortened in direct proportion as the virtual image seems to approach the edge of the cornea.

The most satisfactory relative disposition of the apparatus is that which confines the corneal reflection to the middle third of the cornea. Since, moreover, the middle third of the cornea is the part chiefly concerned in vision, accurate vision is a guarantee of a correspondingly accurate cornea. Only a small percentage or corneal astigmatism is corrected by the lens.

In actual use a direct mathematical elaboration of the results will doubtless seldom or never be necessary procedure. This is partly because absolutely accurate data with respect to the curvature of the cornea and the center of rotation of the eye are difficult to obtain in each individual instance. Partly, however, it is because of the comparative convenience of direct empirical control of the relation between actual displacement of the line of regard and the record. But it is also partly because of other sources of error which our method shares with all other objective methods and which resist easy mathematical evaluation. Probably the most important of these sources of error is the fact that the axis of rotation of the eye is not a fixed point, but a variable (see page 90). Since the actual displacement of any point at the surface of the eye during any given angular displacement of the line of regard depends on the distance of the point in question from the axis of rotation, it follows that any variation in the position of the axis during rotation will distort the objective record of the movement so as to make its various parts of unequal value. If the axis of rotation moved toward the retina, the extent of the eye movement, as measured by the displacement of any point on its surface, would be exaggerated. While if the axis of rotation moved away from the retina, the record would be relatively too small. This will be true of the cornea and its reflection according to the formula above demonstrated. But it will also be true of any direct attachment to the eye.

On these grounds, as well as on others to be exploited presently, it seems probable that the most satisfactory method of evaluating the records of eye movements must be based on the

correlation between certain known eye movements and their records. But under these latter conditions, considerable freedom is permissible in the relative position of the source of light and the general arrangement of apparatus. But it should not be overlooked that, for quantitative accuracy, the only theoretically ideal arrangement is to bring the apparent source of light to the center of the lens and to dispose the object of regard symmetrically about it. Any variation from this arrangement must be regarded as a concession to experimental expediency, and is permissible in quantitative work only under careful empirical control. These conditions of a satisfactory technique may at first sight appear to contrast sharply with the simple technique of Judd's Chinese white. It should be remembered however that we have been speaking of an optimum, and an analysis of the conditions will show an analogous optimum for the relative positions of the Chinese white and the camera.[1] While no method is perfect enough to safely dispense with the empirical control.

§ 2. *The Reliability of the Corneal Reflection Records.*

Under any conditions the question of reliability is a determinate problem, and its answer is the final test of the scientific value of a technique. My experimental answer to the question is based on a double experiment in which eye movements of known amplitude are compared first with the records by the corneal reflection and second with the records made by an artificial attachment to the eyeball.

In the elaboration of the test several difficulties were encountered whose solution has a general as well as a special value. The first difficulty was the general inaccuracy and instability of fixation which was discussed in the first chapter of this monograph. Since the illusion of fixation may persist while the point of regard wanders over an area whose diameter is a full degree of eye movement, and since this fixation area may be asymmetrically oriented with respect to the object of regard, there is no natural standard by which the accuracy of a registering device may be tested. But without some fixed point of

[1] See page 90.

orientation, objective records will merely duplicate part of the inaccuracy of the introspective illusions. This general difficulty is keenly appreciated by all who have given the matter careful attention. Judd, for example, frankly disclaims all implication of an inerrent correlation between any particular point of his record and any particular point of his object of regard. It was enough for his purpose to indicate accurately the form of the eye movement under varying circumstances. It is obvious therefore that my first difficulty is more significant than might be supposed from the immediate context in which it appears. And it is obvious, furthermore, that some solution of the difficulty is a fundamental condition of the reliability of any complete spatial interpretation of any eye movement records. Within a comparatively minute error, such a solution is found in the use of a concurrent after-image control of fixation during some part of the registration. While such control cannot prevent all wandering of the point of regard, it reduces its amplitude to approximately 10′. And under special circumstances the total efficiency of the control may be still further refined. The following test for example was concerned with a series of fixation in the horizontal plane. Vertical lines were chosen as the most efficient fixation marks. And a small round after-image at the fovea, projected in turn on the different lines, gave a delicate and, as the records show, a surprisingly accurate control. The minute fixation movements could not be entirely obviated. But after the line was once properly fixated they were comparatively small, and appear to have been symmetrically oriented. This resulted merely in an increase in the size of the corresponding record without disturbing to any measurable degree its center. One serious charge may be brought against the accuracy of this form of control, when the problem is to locate the absolute line of regard. Namely, it is questionable procedure to assume that the center of the after-image is bilaterally symmetrical with the exact center of the fovea. My answer is a plea of guilty. We cannot be sure of it. On the other hand, in the prolonged fixation of the object which produced the after-image, there is some ground for confidence that the inevitable wandering of

the eye, in so far as it effects the after-image at all will be evenly distributed about a central line which shall pass very near the center of the fovea. But whatever difficulties may be encountered in the use of the after-image as a means of general control, it was eminently satisfactory as a control of the accuracy of a series of six fixations in the horizontal plane, with which alone the present discussion is immediately concerned.

The second main difficulty in the experiment was to obtain an artificial pigment point on the eyeball, at once small enough and brilliant enough to give a record on a fixed plate, comparable with the sharply defined records which are obtainable from the corneal reflexion. This difficulty proved even more refractory than the first. Following Judd's lead and using his experience as fully as possible, I experimented with numberless arrangements which seemed likely to give satisfactory results. The final expedient is not ideal, but it was usable, and may be of some general interest. A local jeweler, Mr. Atwell, to whose interest and help I would hereby tender my public acknowledgment, finally succeeded in turning out and satisfactorily polishing a minute, almost microscopic hemisphere of silver. In order that this minute bead could be handled without getting lost, and also in order to give it an innocuous surface for contact with the eye membranes, I mounted it on a fragment of black tissue paper, approximately 1 mm. in diameter, which, following Judd's discovery, I had previously saturated in paraffine. The presence of this on the cornea, was absolutely painless, without the use of an analgesic. In general, it was entirely unfelt unless in winking it came in contact with the eyelid. This innocent report of its presence proved something of an advantage since it indicated the necessity of a readjustment.

Unless the eye is fairly dry however the whole affair tended to drop slowly. On this account I regard the device as less satisfactory than Judd's bit of Chinese white, whenever the latter is available. But it may serve a general purpose for fixed plate records and falling plate records, where the image of the Chinese white would be too faint or too large. As one might

expect the reflection from this almost microscopic bead, when it was in focus, gave the most delicately beautiful record I have ever seen. But unfortunately when it is not exactly in focus, the general diffusion reduces the photochemical effect so much that the bead for practical purposes requires a great deal higher illumination than does the reflection from the cornea.

The matter of this differential illumination was the third difficulty in the experiment. Since light intense enough for the bead was blinding to vision, and produced marked halation in the image of the corneal reflection, some form of differential illumination was demanded that would be intense enough for

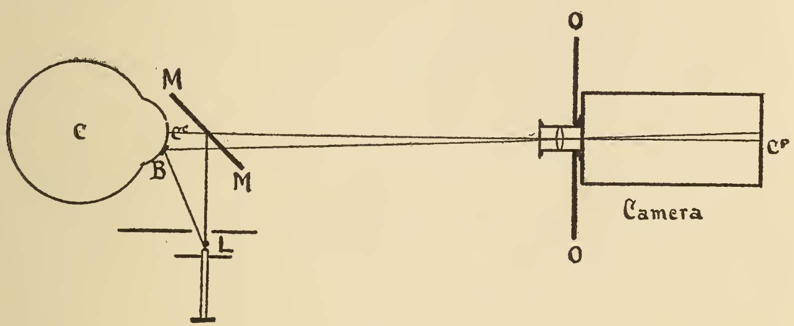

FIG. 6.

FIG. 6 gives a schematic line drawing of the arrangement of the apparatus. *C* represents a cross-section through the eye; *L* is the arc light; *MM* is the transparent mirror; *B* is the minute silver bead; C^c is the apparent position of the corneal reflection; *OO* is a sheet of white cardboard with the six vertical fixation lines.

the bead and greatly reduced for the corneal reflection. This could not readily be produced directly by confining the intense light to any particular zone, since the movements of the two objects left no exclusive zone covering all the positions of either object. The solution of this difficulty was finally effected by the help of my old friend, the transparent mirror. By placing the arc light about 90° to the right of the primary position of the head, the full intensity of the light would be reflected from the edge of the bead, without entering the pupil at all. While a plain glass plate interposed between the eye and the lense at about 45° to the line of regard, threw about one tenth of the

full amount of the light that struck it to the eye. But apparently its source was at the center of the photographic lense. A still further reduction in the apparent brilliancy of the light, was produced by the customary pot-blue glass screens interposed between the arc light and the transparent mirror. The resulting illumination was the mildest I have ever used. It scarcely provided a visible after-image of the arc for controlling the fixation, even after prolonged fixation. But it gave excellent records both on the fixed and on the slowly falling plate. Like the solutions to the two previous difficulties, the device is capable of general application. And I feel justified in recommending it for all quantitative records of the corneal reflection type in which it is desirable to bring the apparent source of illumination to the apparent center of the photographic lense.

The object lines were brilliantly illuminated by the relatively non-actinic light of a 16 c.p. incandescent light situated just above the head.

The resulting plate was a simple record of two lines of dots. As in all first-class records of the corneal reflection no other details of the eye or head are clearly distinguishable. The record alone stands out clear and sharp on an almost plain background. The record was first projected with a lantern onto narrow strips of white paper, which were pricked with a steel stylus as nearly as possible at the center of each fixation record. This gave a total magnification of the eye movements of about 80 times. The distance between the stylus marks was then measured directly by a scale. There are slight sources of error in the method of measurement but careful manipulation reduced them to a negligible quantity relative to the purpose of the test.

The results reduced to movements at the eye are given in the following table in fractions of an inch.

TABLE III.

	Left.		Center.	Right.	
	1–2.	2–3.	3–4.	4–5.	5–6.
	6°24′44″	6°41′14″	6°45′24″	6°41′14″	6°24′44″
Cornea A.	.02131	.02455	.02334	.02455	.021045
Bead B.	.051005	.05693	.05423	.05369	.04425
Theoretical C.	.0536	.0555	.055	.052	.047

Reading from above down the words *Left, Center, Right,* indicate the respective positions of the point of regard from the standpoint of the subject. 1–2, 2–3, 3–4, etc., indicate the movements between the respective fixation marks numbered from one to six beginning at the left. The line 6° 24′ 44″, etc., indicates the value of the respective eye movements in degrees, minutes and seconds measured on a circle whose center is the nominal axis of rotation of the eye. Line *A* gives the actual displacement of the corneal reflection in fractions of an inch, after all enlargement by enlarging camera and projection apparatus has been subducted. It is altogether reliable to three decimal points. Line *B* gives the actual displacement of the bead reflection from the bead in fractions of an inch. These are also accurate to three decimal places. Line *C* gives the thoretical values that should obtain in line *B* under the conditions of the experiment.

The interpretation of the table is neither difficult nor obscure. Doubtless the most striking feature is the relative accuracy of the corneal reflection data for similar angles on either side of the primary position. The records of the equal movements 2–3, 4–5 correspond absolutely. The records of the equal movements 1–2, 5–6 correspond to three ten-thousandth of an inch. The corresponding variations in the bead records are three parts in fifty, instead of none; and seven parts in fifty, instead of three in two hundred. The relative accuracy of the data from the corneal reflection needs no further demonstration. Under proper conditions it is as unimpeachable as any objective record, no more, no less. All the records show the absolute necessity of after-image control where strict accuracy is demanded. Thus it will be seen at once that in spite of the accurate bilateral symmetry of the corneal records, the movement 2–3 which should be slightly less than the movement 3–4 is actually greater by one point in twenty-five. Similarly the drop from the record of the movement 2–3 to the movement 1–2 is all out of proportion to the actual decrease in distance. These variations invalidate unilateral records as far as strict accuracy is concerned except under the control of some empirical standard like the after-image control. But these variations are not accidental.

Neither are they confined to the corneal reflection. Similar variations from the theoretically expected occur in the bead records at exactly the same places and in exactly the same direction, though in slightly varying amounts. Bead records 1–2 and 5–6 are smaller than the theoretical while bead records 2–3 and 4–5 are larger than the theoretical. In either series the records would indicate a real variation of the axis of rotation of the eye, of which the present records are a reasonably accurate indication. The occurrence of the indications coincidently in both records falls little short of demonstration. The data at hand show that the point of rotation of the eye varies approximately .02 in. in the movements under consideration. Doubtless considerably more exact measurements of the variation of the axis would be possible by the use of this joint method, but the present record is not available for that purpose, since neither the rigidity of my head rest, nor the accuracy of fixation, even with the best after-image control is adequate for measurements of the desired refinement.

The bare figures on the bead record taken in comparison with the bare figures of the corneal reflection method would seem at first sight to indicate that the corneal reflection method was the more accurate. As a matter of fact however any implication of comparative inaccuracy in the bead record would be altogether uncritical and unjust. With adequate mathematical interpretation neither has any decided advantage. It must be remembered that the corneal reflection records were made under optimum conditions for bilateral accuracy. A similar optimum for the bead was prevented by the latter's opacity, and the consequent interference with vision which would result if it were placed at the apex of the cornea. Practical considerations forced me, as they forced Judd, to place the bead in an eccentric position at the side of the pupil. In the present instance its actual position was 14° 49′ to the right of the apex of the cornea, about as near as was consistent with clear vision, and at approximately the same relative position that it has in the measurements of Judd and his associates. The simplest mathematical analysis must show that under these conditions, with the camera directly in front of the eye, the two halves of the record could not possibly be symmetrical.

Under the very best conditions if the bead were at the center of the cornea and the camera directly in front the records would be related to the points of regard as sines to tangents. But an eccentric bead never starts fair. When the line of regard passed through the fixation object at the extreme left of the series, the bead was almost directly in front of the camera. When the fixation was at the extreme right the bead would have moved unduly around, away from the camera, by exactly the amount of its eccentric position. Under these circumstances symmetrical displacement of the point of regard, with respect to the camera, will lead to an exaggerated displacement of the bead record at the left and a foreshortening of the record of movement at the right. In all the Yale records the distances were so short that these differences could doubtless be safely ignored, but they would obviously constitute a serious source of error under slightly different circumstances, unless they were subjected to empirical control. Indeed this is the most important lesson of the comparison. No method of objective registration is good enough to neglect either empirical control or theoretical elaboration where strict accuracy is demanded. No objective record can be safely taken merely on its face value.

But even under the most accurate control it is doubtful if we can expect to eliminate errors under 10′. Perhaps that is as accurate as our present needs demand.

§ 3. *The Technique of the Wesleyan Apparatus.*

In view of the considerable changes that have been made in our apparatus since it was first described, and in view of my belief that there is yet a real need for more, relatively accurate data concerning the eye movements, it seems worth while to add a working description of our apparatus in its present form.

The camera is an enlarging camera of fixed length. It is substantially a wooden box 4 ft. long and 6½ in. square, but tapering at the lens end. The lens is a Bausch and Lomb Convertible Protar Series VII., No. 8. Doubtless a cheaper lens would answer most every purpose, but the above has abundantly justified its presence in the laboratory. At the back of the camera box in place of the ordinary plate holder is an adapter

by which an ordinary double plate holder may be used for fixed plate exposures (Stratton's method); or my falling plate device may be instantly substituted at will without changing the focus, or otherwise disturbing the apparatus. This is a convenient feature and is recommended.

A crucial part of all registering devices is the head-holder. While that in possession of the Wesleyan Laboratory is fairly satisfactory, I have yet to find one that will hold the head absolutely rigid without discomfort. The only accessible point of contact with the skull is the upper teeth. All other points of contact are more or less compressible and elastic. But even the teeth are not rigid. For myself, generally, and in the above experiments, I use three points of contact: (1) An upper jaw rest, consisting of a strip of whitewood and in which a wedge has been cut which latter just fits into the indentation between the two upper incisors. The strip of wood is carried on a metal clamp, attached to a ½ in steel rod, which in turn is firmly fastened to the side suports of the head-rest. (2) The second point of contact is a wooden nose bridge, which is cut to fit the bridge of the nose, and is attached to a cross piece, which latter is clamped to the main uprights. (3) The third rest is a mastoid rest, consisting of a wooden block adjustable to various positions and capable of being firmly clamped to the main head-rest. I formerly used a mastoid rest on each side of the head but I found it awkward and of little additional help. This form of head rest is no guarantee of a motionless head. On the contrary it presupposes some intelligent control of the head and a consistent effort to maintain an even pressure against the three supports. That in spite of these precautions head movements of no inconsiderable amplitude still persist indicates the necessity of a head record.

Apparently the best illumination for the corneal reflection is the arc light. Following Holt's suggestion, I use an alternating current, and consequently get a serviceable time line combined with the record. The light is always stopped down by one or more thicknesses of pot-blue glass. This produces a highly actinic light of low physiological intensity. Such a light gives fine sharp lines on the negative and is always available. In both respects I find it better than direct sunlight which

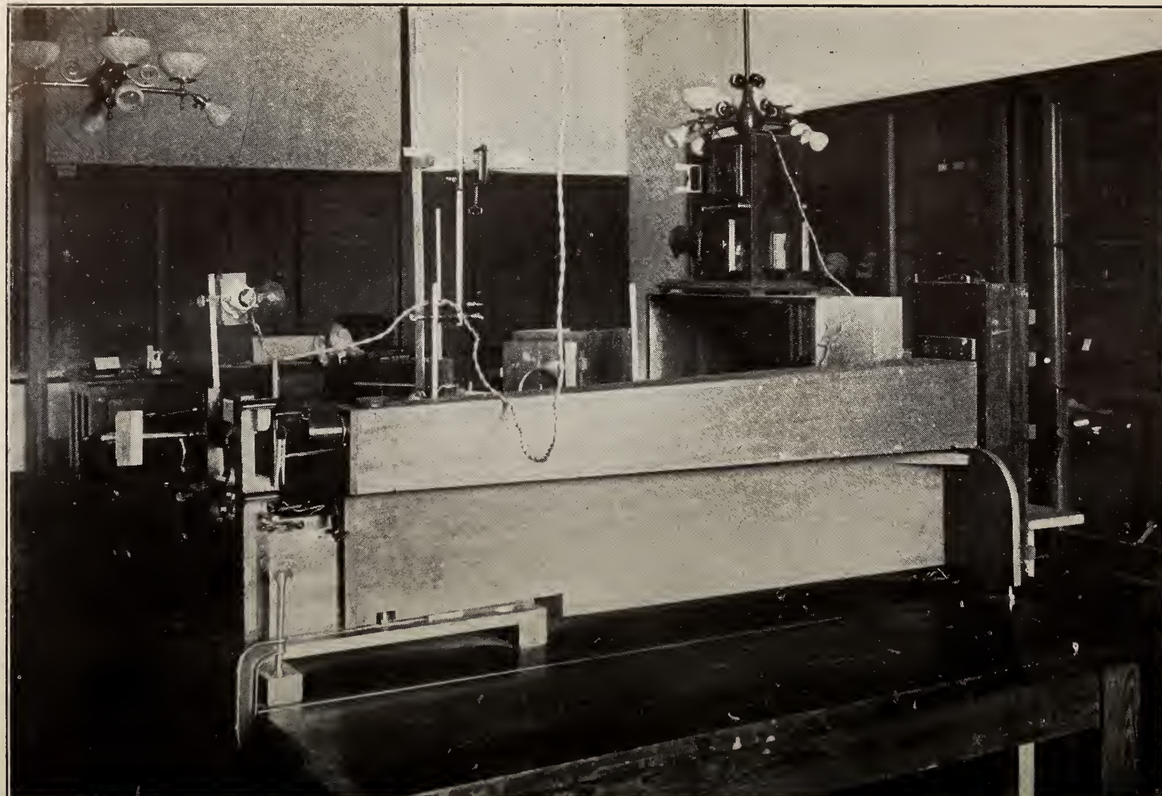

Plate III.
Photographic Recording Apparatus for Registering the Eye Movements.

I formerly used. Undoubtedly the best arrangement of the light is to use the transparent mirror, described on page 87. Practically similar results may be obtained by the use of a small total reflecting prism directly in front of the lens and just below its free opening. Plate III. gives a picture of the entire apparatus in position.

We do not regard any record as wholly satisfactory unless the lines are minute hair lines. Big, black, broad lines look good to the naked eye, but do not permit accurate reading. The best lines do not show at all in the dark room and all the plates are developed by time.

Doubtless the most important control in the whole technique is the concurrent registration of the head movements. For this purpose I use a spectacle frame carrying small beads, much like Judd's. Under these circumstances the alternating current dots on a rapidly falling plate may on demand reach the definiteness of kinetoscopic pictures in which all the useless parts have been eliminated. The relative position of spectacle dots and corneal reflection dots will indicate vertical as well as horizontal movements. In the case of head movements the axis of the rotation of the head is so far behind the apex of the cornea that beads and cornea are practically one system for the minute movements of the head which are involved. The relation between them is illustrated by the lower part of Plate I., Fig. 2. Since the movement of the plate in this record was unusually slow to produce a long exposure the individual dots of the alternating arc are indistinguishable. But it is obvious that head line dots in connection with records like Plate I., Fig. 3, would give adequate data for triangulation readings. This device is a direct plagiarism of Judd's more elaborate kinetoscope pictures, but at present I have no adequate tests by which I can claim anything like equal accuracy in the registration of vertical eye movements on my falling plate. A momentary interruption of the arc light each second, by the rapid movement of a pendulum attachment greatly facilitates the reading of the plates by emphasizing corresponding points. In all cases where it was necessary to record the vertical movements of the eyes I have hitherto preferred Straton's device of a fixed plate. The only difficulty in its use is the lack of both time and

head lines. The latter difficulty is a serious one but wherever a trained subject is available with special precautions to keep the head steady these records are reliable to 15′ as is demonstrated by the following control. With the plate moving so slowly that several minutes elapsed between the beginning of the experiment and the end I took a record of the fixations, with after-image control, of three series of movements across the six fixation lines, which served as fixation objects is the main test of reliability, page 85 fol. The last part of the record was the successive fixation of two of the lines five times repeated and this is reproduced enlarged in Fig. 7. All the fixation lines are crooked and show the fact, that was perfectly obvious by means of the after-image control, that the fixation was far from perfect. I persisted, however, in looking at the fixation mark until the after-image control indicated a moment of comparatively accurate fixation. That was the signal to move to the next line. So that the last moment of the record of any fixation was true as far as was possible under the circumstances. The successive voluntary displacements caused in general more irregularity than was present in the earlier test. Comparing the true fixation moments, which are represented in the reproduction at the top of each line, it was found that there was a slight displacement of the successive positions. These may be accounted for in some measure by the imperfections of the after-image control, but they are primarily head movements. In the fixations herewith reproduced the greatest error in any movement as represented by the variation from the mean was one twenty-fifth of the total movement, *i. e.,* 15′ of eye movement. The total displacement of the head was under 30′ and the maximum displacement for the entire unusually long series would give a maximum error in the readings if interpreted directly as variations in the position of the eye of less than a degree. This rather severe test may be taken to indicate the practical possibilities in the use of the recording apparatus without a head line control. Where the latter is permissibly dropped there is immense saving of time

FIG. 7.

and plates, as a much larger number of records may be taken on one plate. In the extent of any given eye movement the probability of error will be below the error of measurement. In a succession of fixations the error ought not to be more than $1°$. In general the maximum errors will be in the form of the slow drifts, which we observed in considering the relation of the head and eye movements. The smaller errors may be sharp and rapid corresponding to the pulse and respiration curves. Undoubtedly subjects vary in their control of the head movements and the variation in any given case should be tested out with the aid of the normal head lines.